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PART I.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

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It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the Chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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PHILOSOPHICAL TRANSACTIONS.

I. *A Second Paper on Hygrometry.*

By J. A. De Luc, *Esq. F. R. S.*

Read December 9, 1790.

IN a Paper which I had the honour to preſent to the Royal Society in the year 1773, I ſketched the following propoſitions, as fundamental for the conſtruction of an *hygrometer*. 1ſt, That *fire*, conſidered as the cauſe of *heat*, was the only agent by which *abſolute dryneſs* could be *immediately* produced (§ 5.). 2d, That *water*, in its liquid ſtate, was the only ſure *immediate* means of producing *extreme moiſture* in hygroſcopic bodies (§ 8. and *ſeq.*). 3d, That there was no reaſon, *à priori*, to expect, from any hygroſcopic ſubſtance, that the meaſurable

effects produced in it by *moiſture* were proportional to the intenſities of that cauſe; and, conſequently, that a true *hygrometrical* ſcale was to be a particular object of inquiry (§ 2.). 4th, laſtly, That perhaps the comparative changes, of the *dimenſions* of a ſubſtance, and of the *weight* of the ſame or other ſubſtance, by the ſame variations of *moiſture*, might lead to ſome diſcovery in that reſpect (§ 72.). The ſame propoſitions will be the ſubject of this Paper.

Of abſolute dryneſs.

1. An *hygroſcopic* body, which is not brought into contact with any other body *drier* than itſelf, cannot loſe any part of its *moiſture* but by *evaporation*; and if this is intirely produced by *fire*, there may be ſuch a degree of *heat* as will cauſe the total *evaporation* of that *moiſture*. This is the principle on which the above firſt propoſition was founded; but at the ſame time I mentioned, that I had not brought it into practice, becauſe of the impoſſibility of ſubmitting the ſubſtance of the hygrometer to ſuch a degree of heat. However, I ſoon removed that difficulty by conſidering, that the degree of *heat* neceſſary to produce *extreme dryneſs*, might be applied to ſome ſubſtance that could bear it; and that *dryneſs* be tranſmitted to the *hygrometer*, by incloſing it with that ſubſtance in a proper veſſel. The ſubſtance I choſe was *potaſh*; and I prepared, for this and ſome other hygrometrical purpoſes, an apparatus which was made by Meſſ. NAIRNE and BLUNT in 1776. But a new objection ſtopped me again in that purſuit, and led me for ſome time to a very great and now almoſt uſeleſs labour. The degree of dryneſs produced by *potaſh* ſo uſed, could be only proportionable to the degree of *heat* that it had received; and

not conceiving yet any known limit to the intenſity of *heat*, I could not expect any limit to *dryneſs*, nor even a fixed degree of it.

2. I remained at that point, with however a *comparable* hygrometer laboriouſly conſtructed, till I came to conceive, that *heat* muſt be at its *maximum* in a body, when it is *incandeſcent*; which opinion I have explained in my work, *Idées ſur la Météorologie*. From that firſt idea I ſoon after concluded; that every *hygroſcopic* ſubſtance, which could retain that property after having been brought into *incandeſcence*, would anſwer my firſt purpoſe. The following is the theory reſulting from the whole of the above conſiderations. 1ſt, The *hygroſcopic* ſubſtance which has the moſt capacity for *moiſture*, and receives it the moſt readily, being placed in any quantity in a given ſpace, cannot bring that ſpace to a degree of *dryneſs* greater than its own; and if that degree is undetermined, it cannot afford any *fixed point* for the *hygrometer*. 2d, The *hygroſcopic* ſubſtance which has the ſmalleſt capacity for *moiſture*, and is the ſloweſt in receiving it, if it is really reduced to *extreme dryneſs*, will have the power of producing it in a given ſpace, provided its ſmall capacity be compenſated by a greater quantity, and its ſlowneſs by more time. 3d, Every *hygroſcopic* ſubſtance, which may be brought to *white heat* without loſing its property, is fit to produce *extreme dryneſs* in a cloſe ſpace. 4th, It is indifferent for that purpoſe, that the ſubſtance uſed be of the claſs which has a chemical *affinity* with *water*, it being ſufficient that, after having been reduced to *extreme dryneſs*, it be ſtill capable of receiving it from the ambient *medium*, as may every porous ſubſtance. 5th, But for the practical purpoſe of fixing the point of *extreme dryneſs* on *hygrometers*, ſuch a ſubſtance muſt be choſen as, with a great

capacity for *moiſture*, receives it but ſlowly: as by that firſt property it may be taken in leſs quantity; and by the laſt, it will be leſs ſubject to acquire a ſenſible quantity of *moiſture* in the time neceſſary for the operations.

3. *Pot-aſh* and ſome other alkaline ſubſtances afforded the firſt of thoſe properties, but not the laſt; and I had not fixed on any ſubſtance, when, being at Birmingham in the autumn of 1782, Mr. JAMES WATT informed me, that his friend Dr. BLACK had found in *quicklime* a great capacity for *moiſture*, and much ſlowneſs in retaking it: this he knew, by having kept a long time the ſame *lime* in a cloſe veſſel, for drying ſalts and capillary tubes for thermometers. Theſe were the very properties I wanted for my purpoſe, which thereby I executed as ſoon as I came home. I made thoſe firſt operations in ſmall glaſs veſſels, uſing old *lime*, which I brought again to *white heat* every time I uſed it. Theſe firſt trials agreed with my theory in its firſt point; that of producing conſtantly the ſame degree of *dryneſs*: as for the ſecond, namely, whether that degree was *extreme*, it depended on other experiments.

4. Being ſure then of a fixed degree of dryneſs, the number of experiments I undertook made me wiſh for a means of avoiding the frequent repetitions of bringing again my *lime* to *white heat*; and having found one which has ſucceeded, I am going to deſcribe the apparatus. The veſſel, fig. 1. is of tin, 3 feet high and 1 in diameter. A glaſs plate *a*, *a*, *a*, *a*, is fixed at the top, forming a vertical ſection of the cylinder at 1 inch diſtance forwards from the axis. A woven braſs-wire cage *b*, *b*, *b*, *b*, is fixed in the veſſel through its diameter, in order to keep a ſpace for the inſtruments; for the ſame purpoſe it is open at the top, and alſo oppoſite the glaſs, where the dials of the inſtruments are to be ſeen. For my experiments,

ments, which required inſtruments of various ſizes, I made that cage 18 inches high and 2 deep; but it may be much ſmaller for common hygrometers. The whole veſſel, except that ſpace, has been filled, through the openings *c*, *c*, with *quicklime* taken from the kiln, and ſuffered only to loſe the red heat; after which the openings were covered with heaps of the ſame lime, which abſorbed the moiſture of the air entering the veſſel while it was cooling, and then the openings were ſhut with tin plates and putty. The top of the veſſel has four ſquare openings *d*, *d*, *d*, *d*, correſpondent to the wire cage, for the introduction of the inſtruments, which are hung to hooks. I uſe a hooked wire for putting in or taking out the inſtruments, to avoid bringing my fingers near the openings. Theſe are kept ſhut with tin plates and putty: I never open but one at a time, which I leave open as little as poſſible; and to prevent the introduction of the external air in thoſe ſhort operations, I make them as nearly as poſſible at the ſame temperature, which being 60° of FAHRENHEIT may be obtained in every ſeaſon. With theſe precautions, and alſo by moiſt air being lighter than dry air, there is ſcarcely any moiſture introduced in the veſſel except by the inſtruments themſelves.

5. This application of my method afforded me a very ſtrong confirmation of the practical fixity of the point of *dryneſs* produced in that manner by *lime*; for the apparatus was different from the former ones; 1ſt, by the quantity of the *lime*; 2d, by the *lime* having been put very hot into the veſſel, while, when I uſed glaſs veſſels, I had ſuffered it ſometimes to cool down to 60°; 3d, by that *lime* being of the firſt calcination, inſtead of old *lime* brought again to white heat; and all theſe differences produced no ſenſible effect on the point of *dryneſs*. Since that time Meſſ. NAIRNE and BLUNT, Mr. HURTER, and Mr. HAAS,

HAAS, have made apparatuſes of the ſame kind, and I have made myſelf ſome others of different ſizes and ſhapes; and they all produce ſenſibly the ſame degree of *dryneſs*.

6. The deſcribed apparatus was ready in the month of October, 1787, and I put in it one of my firſt *hygrometers*; which, in a few days, came to its fixed point of *dryneſs*, and there it has remained ever ſince, though I have opened the veſſel above four hundred times. That degree of conſtancy, much beyond my expectation, has enabled me to make a variety of experiments, which elſe had been next to impoſſible: it proceeds partly from the great capacity of *quicklime* for *moiſture*, which I ſhall determine hereafter; and partly from its ſlowneſs in receiving it; which circumſtances, added to the ſmall ſize of the openings, to their being at the top of the veſſel, and to the care of putting in and taking out the inſtruments nearly at the ſame temperature, prevents the *lime* from acquiring any ſenſible degree of *moiſture* during theſe operations.

7. I did not truſt at firſt the apparent continuance of the ſame degree *of dryneſs* in that veſſel. At the end of nine months of frequent uſe, I began to fear, leſt the *whalebone*, of which the *ſtandard hygrometer* is made, had been impaired; and I took it out, to try its point of *extreme moiſture* (of which I ſhall ſpeak hereafter;) but it came exactly to that point, and when put again into the lime-veſſel it returned where it ſtood before. I have repeated many times that trial, with the ſame reſult; the laſt time was at the end of three years, when, inſtead of a loſs of expanſibility in the *whalebone*, I found a ſmall increaſe, but probably accidental; it went a little farther than its point of *extreme moiſture*, and came back to its conſtant point of *dryneſs*.

8. The

8. The principles of hygrometry being now my only object, it would not be proper to enter into particulars on its practical part; but I ſhall here mention for once, that the ſteadieſt *hygroſcopic* ſubſtances are ſubject to anomalies: for inſtance, after an hygroſcope has remained fixed in *water* for many hours, if it is taken out, ſuffered to dry a little, and then put again into *water*, it may ſometimes happen to overpaſs that point. In the ſame manner, after an hygroſcope has been long fixed in the lime-veſſel, it may happen alſo, that in taking it out only for a quarter of an hour, and putting it in again, it will move a little farther than it was before. Again, if in taking it out of the *lime-veſſel*, where it had long remained fixed, it is put into *water*, and then back into the *lime-veſſel*, it may happen, that it will fix itſelf a little ſhort of its former point, and never move thence, except by repeated great variations of heat; but if, when it ſhews that diſpoſition, it is taken out for a ſhort time, and put in again, it will then attain its uſual point. This was the caſe in the laſt trial of my *ſtandard*. Laſtly, the ſame anomalies may take place at every other point of the ſcale of every hygroſcope, only more or leſs according to the ſubſtances; ſome of which, for that reaſon, cannot be uſed for practical hygrometry.

9. Thoſe anomalies of the ſteadieſt *hygroſcopic* ſubſtances, will probably prevent our ever having in the *hygrometer* an inſtrument nearly ſo exact as the *thermometer*; and this I was to premite, that when I mention the reſults of particular *hygroſcopic* experiments, it may be underſtood, that they have only the degree of exactneſs that belong to their claſs. Luckily thoſe anomalies are yet of no conſequence for the great objects of *hygrology* and *meteorology*; the preſent ſtate of *hygrometry* being ſufficient to excite on thoſe objects, queſtions of great importance for natural

natural philoſophy. And in the mean time thoſe anomalies are very intereſting in themſelves; as, from their laws, they ſeem to point out ſome modification of *cohæſion*, as being the immediate cauſe of *elaſticity* in *ſolids*. If I can find time to put in order a number of obſervations and experiments I have made in that reſpect, I intend to make it the ſubject of a Paper, in which I ſhall examine, from a general reſult of thoſe phænomena, the comparative uſe of *weights* and *ſprings* for keeping ſtretched the *hygroſcopic* ſubſtance in *hygrometers*.

10. After *fixity* in the degree of *dryneſs* produced by *lime* in the manner I have explained, the next point to be examined in reſpect of my theory, was, if the *nature* of the ſubſtance brought to a *white heat*, had any influence on the degree of *dryneſs* thereby produced; and in order to try at once the effect of a very great diſparity, I choſe ſuch a *ſand-ſtone* as is not affected by *acids*, and ſtrikes fire from ſteel, before and after having been *incandeſcent*. The firſt experiment I made was with a view of finding the comparative capacities for *moiſture* between that *ſtone* and *lime*. For that purpoſe I took ſuch pieces of them as might be readily reduced to half an ounce while *incandeſcent*; which being done, I put them into braſs cups, fitted to a ſcale, and I incloſed them under a glaſs veſſel inverted over *water*. I weighed thoſe ſubſtances from time to time; each of them continued to acquire *weight* during five weeks; at which time the *ſand-ſtone* had gained $\frac{1}{210}$ part of its original *weight*, and the *lime* $\frac{110}{210}$: this laſt was at that time all cracked and fallen in ſmall fragments, eaſily reduced to powder; the *ſand-ſtone* ſtruck fire as before. I next prepared a cylindrical tin veſſel, 10 inches in diameter and 14 inches high, with a glaſs top, which I filled with fragments of that *ſtone*, treated as the *lime*; and when it was cooled, I

put into it an *hygrometer*, whoſe fixed point of *dryneſs* has been taken in the *lime* apparatus; and in five weeks it was fixed to the ſame point. This is a demonſtration, that the *nature* of the ſubſtance does not interfere with the *degree* of *dryneſs* produced, and that *incandeſcence* is the only cauſe of its *fixity*.

11. Laſtly, in reſpect of *hygrometry*, a degree of *dryneſs* thus determined might have been ſufficient; but for *hygrology*, and even for natural philoſophy in general, it was deſirable to diſcover if that *fixed degree* of *dryneſs* was alſo *abſolute*: and the following are the conſiderations which directed me in that enquiry: if *evaporation* is produced by *heat* only, and if *incandeſcence* is the *maximum* of *heat*; an *hygroſcopic* body, which is brought to *incandeſcence*, cannot contain any *evaporable* water; and if that body has ſuch a maſs, as to be capable of abſorbing all the water *evaporated* in a certain ſpace, without acquiring any meaſurable *moiſture*, that ſpace may be called *abſolutely dry*. Now, if an *hygroſcopic* ſubſtance which is incloſed in that ſpace, contains any ſenſible quantity of *evaporable* water, when *heat* increaſes, that ſubſtance muſt loſe a part of its *moiſture* in the *medium*, and take it back by the diminution of heat. Conſequently there was a means of diſcovering, if *hygroſcopic* ſubſtances, reduced to the above degree of *dryneſs*, ſtill retain a ſenſible quantity of *evaporable* water; it was that of obſerving their *weight*, by changes of heat: and from theſe previous conſiderations I made the following experiments.

12. I hung ſucceſſively to a very ſenſible *beam*, ſhewing the changes of *weight* by an *index*, different ſorts of vegetable and animal ſubſtances, the *beam* being incloſed in a glazed tin-veſſel, containing a ſufficient quantity of *quicklime*; and during the operations, I produced from time to time great changes in the *temperature* of the veſſel. As long as theſe ſubſtances

retained a ſenſible quantity of *evaporable* water, the increaſe of *heat* made them loſe ſome *weight*, which they regained partly when the *heat* returned to the ſame point. But that effect diminiſhed by degrees; and, at laſt, a change of 30° of *Fahrenheit* did not produce any ſenſible change of *weight* in thoſe ſubſtances, though they were ſuch as had a great capacity for *moiſture*. An *hygrometer* placed near the *beam*, was then at the point taken in the *lime-veſſel*. That ſingle experiment confirms all the previous conſiderations from which I had expected *abſolute dryneſs* from *incandeſcence*.

Of extreme moiſture.

13. The ſecond propoſition I had ſketched in my firſt paper, is this "that *water*, in its liquid ſtate, is the *only* ſure immediate "means of producing *extreme moiſture* in *hygroſcopic* bodies."

14. *Moiſture*, the nature of which we are firſt to determine, may be conſidered in three different caſes.—1ſt, In ſubſtances which have an *affinity* with *water*; by which their moleculæ and thoſe of *water* may unite, and form a new compound.—2dly, In ſubſtances which have no *affinity* with *water*, but to which *water* has a tendency to adhere; by which cauſe it enters their *capillary pores*.—3dly, In the *medium*, or ſpace free from viſible bodies. I have not undertaken to diſcover what, in the firſt of theſe caſes, might properly be called *moiſture*, and its *degrees*; as I foreſaw great difficulties in that undertaking, which beſides was unneceſſary to my principal purſuit: therefore I come immediately to the ſecond caſe.

15. When I wrote my work *Idées ſur la Météorologie*, having not yet made ſome experiments I had in view to verify the opinion I entertained, that vegetable and animal ſubſtances, as well

well as the porous mineral ones, received *water* merely by the faculty of *capillary pores*, I uſed ſometimes the expreſſion *hygroſcopic affinity*, in treating of the *hygoſcropic equilibrium*; but before that work was come out of the preſs, having converſed on that object with Dr. BLAGDEN, and found him inclined to that opinion, I had time and opportunity to expreſs it, in § 276, as follows: "There are reaſons to doubt, whether ſome "of the ſubſtances, which ſhare amongſt them the *water* diſ- "ſeminated in a ſpace, do not *ſuck* it, by a faculty ſimilar to "that of *capillary tubes*, without any *chymical affinity* with "*water*." That opinion will be now confirmed by the following experiments.

16. 1ſt. EXP. *Sugar* has an *affinity* with *water*, and no ſenſible one with *alcohol*: however, a lump of *ſugar* will *imbibe* this laſt *liquid* as readily as the firſt. Conſequently, *water* is not *imbibed* by ſugar in conſequence of their *affinity*; ſince *alcohol* is alſo *imbibed*: they both aſcend in *ſugar*, by the faculty of its *capillary pores*, as they do in *ſand-ſtone* or in *ſponge*. But when *water* has thus entered *ſugar*, it *diſſolves* it; which then is a *chymical* effect; whereas *alcohol* evaporates, and leaves the *ſugar* ſenſibly as it was before.

17. 2d. EXP. If *water* penetrated *hygroſcopic* ſubſtances of the vegetable and animal kinds, by an *affinity* with them, it would not be natural to expect, that other *liquids*, which do not ſhew an *affinity* with the ſame ſubſtances as *water*, ſhould penetrate the above-mentioned ſubſtances. Being led by that conſideration, I made two *hygroſcopes* of different elaſtic *animal* ſubſtances; and after having marked the point where they ſtood in *water*, I immerſed them ſucceſſively in *alcohol* and in *ether*; by which *liquids* they were expanded nearly as much as by *water*, and they contracted as much in coming out of them.

In thoſe experiments a ſingular phænomenon happened in both hygroſcopes. The firſt effect of the immerſion of thoſe *animal* ſubſtances in *alcohol* (and I ſuppoſe it would have been the ſame with *vegetable* ones) was *contraction*, ſoon followed by *expanſion*; and when they came out into the air, the firſt effect was *expanſion*, ſoon followed by *contraction*. The cauſe of that reciprocal phænomenon is undoubtedly the *affinity* of *alcohol* with *water*. In the immerſion, ſome of the *moiſture* came out of the ſubſtance, to unite with the ſurrounding *alcohol*; by which loſs of *water*, a *contraction* took place in the ſubſtance, 'till it came to imbibe the *alcohol* itſelf. In the immerſion, ſome of the *moiſture* of the air, uniting immediately with the *alcohol* retained by the ſubſtance, expanded it to the ſame degree as if it had been in *water*; after which, the *alcohol* evaporating, the ſubſtance contracted. Thoſe phænomena did not happen with *ether*; this not uniting readily with *water*; but it expanded thoſe ſubſtances as much as *alcohol*, and nearly as much as *water*. From thoſe phænomena we may conclude, that the penetration of animal ſubſtances by *water*, and conſequently by *moiſture*, is produced, as that of *ſugar*, *ſand-ſtone*, and every other porous ſubſtance, by the faculty of *capillary pores*, without any *affinity* between them and *water*,

18. 3d. EXP. In that theory, of a mere *imbibition* of *water* by *hygroſcopic* ſubſtances of the *elaſtic* kind, a circumſtance, which ſeems to point out *affinity*, was to be explained; it is that of the *hygroſcopic equilibrium*. In view of that object, I made the following experiments, not new in themſelves, but directed to my purpoſe. I took ſome glaſs tubes, of different ſmall bores, which I firſt bent in the ſhape of ſyphons; after which I cut them in the middle of the bent part. This was to enable me, to bring into exact communication the lower end of two tubes, though

though held in a vertical position, as an inverted syphon (Fig. 2.) The following are the experiments.—1st. Exp. When a column of the liquid had ascended in one of these tubes; if I applied to it an empty tube of the same bore, the *liquid column* divided itself equally between them.—2d. Exp. When the empty tube was of a smaller bore, that *column* rose more in it, than it sunk in the other; and the contrary happened when the empty tube was of a larger bore.—3d. Exp. When some more *liquid* was supplied to the united tubes, it rose in both, in proportion to the respective heights of the former *columns*.—4th. Exp. When a superabundant quantity of *liquid* was supplied, it rose to a *maximum* in each tube, and the heights of the columns increased in some proportion with their former heights.

19. These known facts have a clear analogy with the *hygroscopic equilibrium* in *elastic* substances.—1st. Case. When the quantity of *liquid* common to *capillary tubes* is not sufficient for them to receive their respective *maximum*, they share it between them, and the *equilibrium* takes place, when there is, in each of them, the same ratio between its specific *capillary power* and the weight of the raised column. In the same manner; when the quantity of *water* disseminated in a space, is not sufficient, for several hygroscopic substances to receive the *maximum* of *water* which they can contain in their *pores*, they share it amongst them: and the *equilibrium* is produced, when there is in each of them the same *ratio*, between its specific *capillary power*, and the resistance of their *pores* to be more dilated.—2d. Case. When there is a superabundant quantity of *liquid* common to some *capillary tubes*, each of them receives its *maximum*; which is determined by an *equilibrium*, between its total *capillary power*, and the *weight* of the raised *column*. In the same manner, when there is a superabundant quantity of *water* common

to

to ſeveral *hygroſcopic* ſubſtances, each of them receives its *maximum*; which is determined by the *equilibrium*, between their total *capillary power*, and the reſiſtance of their *pores* to be more dilated. That final *equilibrium*, which, from its very nature, cannot be overpaſſed in any *elaſtic* ſubſtance properly uſed, determines the ſpecific *capacity* of thoſe ſubſtances for *moiſture*.

20. *Moiſture* then, conſidered in *porous* bodies not *ſoluble* by *water*, may be defined, "A quantity of *water*, which is "*inviſibly* contained in their *pores*; without any other connec-"tion with their ſubſtance, than that which it has with the "*glaſs* of the *capillary tubes* into which it has aſcended."

21. We may ſee now whence proceeds the *hygroſcopic equilibrium* between *elaſtic* ſubſtances incloſed in a ſpace, either filled with *air* or deprived of it. In this explanation it is unneceſſary to determine, how *water* is *inviſibly* diſſeminated in ſpaces free from viſible bodies, therefore I ſhall not enter here into this ſubject; that diſſemination is a fact admitted in every hypotheſis, conſequently the *medium* is only to be conſidered as the ſtock and ſtandard of *moiſture*. By the cauſe, whatever, of *evaporation*, *hygroſcopic* ſubſtances loſe or gain *water* in the *medium*, according to its degree of *moiſture*, till they are in *equilibrium* with it; which implies the *equilibrium* amongſt themſelves according to the laws reſulting from their own nature.

22. Now *moiſture*, in a general ſenſe, will appear to be, "a "quantity of *inviſible water*, either *evaporable*, or *evaporated*." And from that definition, the *maximum* of *moiſture* will exiſt, when, "every circumſtance remaining the ſame, no more "*water* can be admitted in a ſpace, without becoming *viſible*; "on *ſolid* bodies, by their ſurface being *wet*; and in the *medium*,

"by a ſpontaneous *precipitation* of *water*." Laſtly, as immerſing ſolid *hygroſcopic* bodies in *water*, or expoſing them in a *medium* where there is an actual *precipitation* of *water* (as in a *fog*) is an effectual means of furniſhing their *pores* with the whole quantity of *water* they can *imbibe*; it is evidently a *ſure* means of producing *extreme moiſture* in them: and this point cannot be overpaſſed, neither in *water*, nor in *fog*, ſince it depends upon the *reſiſtance* of the *pores* to further dilatation by the mere introduction of *water*; but it muſt be attained in an *hygrometer*, to fix its point of *extreme moiſture*.

23. When formerly I had fixed upon that method for procuring to my firſt *hygrometer* a true point of *extreme moiſture*, it occurred to me, that the *temperature* of *water* might influence ſenſibly the expanſion of its *ivory* tube; and in order to diſcover if it was ſo, I made ſome experiments, related in §§ 104 &c. of my former paper, the reſult of which was, that the *temperature* of *water* had a ſenſible effect on the expanſion of *ivory*. But ſoon after I diſtruſted ſome modifications of that complicated *hygrometer*; and eſpecially this particular reſult.

24. I then changed that firſt method; which conſiſted in meaſuring the changes of capacity of hollow cylinders, into that of meaſuring the changes in length of *hygroſcopic* ſubſtances; and for ſome preliminary experiments on many of them, I made particular *frames*, in which, by a combination of *glaſs* and *braſs*, the effects of *heat* on thoſe materials compenſated each other; by which means the *indices* of thoſe inſtruments were only affected by the modifications of the *hygroſcopic* ſubſtances tried in them. I have mentioned theſe *frames* in a paper on *pyrometry*, printed in the *Phil. Tranſ.* for 1778. With theſe *frames* I firſt tried *ivory*, in *water* of different *temperatures*; and

and I found a very little difference in its *expanſion*, comparatively with what had appeared from my firſt experiment. Then, continuing the ſame trial on various ſubſtances, I found the effect of different *temperatures* of *water* very ſmall in general; and even in ſome ſubſtances, as *deal* taken lengthwiſe, and *hemp*, I could not aſcertain any.

25. Theſe experiments led me to think, that the ſmall variations produced by *heat* in hygroſcopic ſubſtances dipt in *water*, were not *hygroſcopic* modifications, but the mere effects of *heat*, by the ceſſation of all *hygroſcopic* modifications; theſe having then attained their *maximum:* which is a diſcrimination of effects, that I had vainly attempted to produce by other means. When afterwards I had found the method of producing *extreme dryneſs*, I made a lime apparatus, for the purpoſe of repeating in it the ſame experiments with my *compound frames*; and I found that theory confirmed, by the effects of *heat* in that apparatus being nearly the ſame, on the ſame ſubſtances, as when they were in *water*.

26. From the whole of the foregoing experiments there cannot remain any doubt, that *water*, in its liquid ſtate, is a *ſure* means of fixing the point of *extreme moiſture* on *hygrometers*. Particularly, in reſpect of *elaſtic* ſubſtances, as *ivory*, *quill*, *whalebone*, all ſorts of *wood*, and a number of others which I have tried, the laſt experiments in *water* of different *temperatures*, afford an immediate proof, that their faculty of *ſucking* water has a fixed limit, proceeding from a final *reſiſtance* of their *pores*, to be more dilated by the introduction of *water*. Conſequently, their *utmoſt expanſion* is a true ſign, that *moiſture* is *extreme* in them; which point cannot be exceeded. But my propoſition extended farther: I had ſaid, that *water* was the *only* certain means of obtaining immediately the point of

extreme *moiſture* on *hygrometers*; and this is a moſt important queſtion, both of *hygrometry* and *hygrology*, which remains to be examined.

On the maximum of evaporation, *and its correſpondence with the* maximum of moiſture *in a* medium.

27. Since *moiſture* conſiſts in *inviſible* water, an *exceſs* of *water* is the only immediate means of aſcertaining that the *maximum* exiſts; as, if a reſervoir is above our reach, the only means of knowing if it be *full* of water, is when it overflows. From that principle, a *fog* gives the point of *extreme moiſture* on *hygrometers*, like *water* itſelf; becauſe it covers very ſoon the *hygroſcopic* ſubſtance with a coating of *water*: ſometimes even it expands it a little more than an immediate application of *water*; but this belongs to an object that I have waved before, as relating to ſome modifications of *elaſtic ſolids* (§ 8.). No other means then but an *exceſs* of *water* over the ſurface of the *hygroſcopic* ſubſtance of the *hygrometer*, can aſcertain that it is arrived at its point of *extreme moiſture*; and the firſt immediate demonſtration I ſhall give of it, will be afforded by *dew*; a very uncertain, though apparently certain ſign of *extreme moiſture* in the *air*. We ſay, that there is *dew*, when ſome ſolids expoſed in the open air in a clear evening are *wet*; but if that was the effect of a *precipitation* of *water* happening in the *air*, all the ſolids thus expoſed would be *wet*; which is far from being the caſe; conſequently, that phænomenon muſt proceed from ſome particular cauſes, by which, though no *water* is yet diſpoſed to abandon the *medium*, it gathers on ſome particular ſolids. It is very long ſince the phænomena of *dew* have perplexed natural philoſophers; and they were the firſt

which I ſtudied in the beginning of my reſearches in meteorology; but all that I concluded from my experiments and obſervations was, that we could not underſtand thoſe phænomena without firſt having a ſure *hygrometer*. This is the reaſon why, ſoon after I had made my firſt *hygrometer*, I expoſed it in the open air in the country, ſuſpended very little above the graſs, from the morning of a fine day to the time of *dew* in the evening; the *graſs* grew *wet*, and the *hygrometer* remained at a great diſtance from the point which had been fixed in *water*. I have related that experiment in § 91, of my firſt Paper.

28. When I had made hygroſcopes of various ſorts of *ſlips*; for inſtance, of different *woods* and of *whalebone*, cut acroſs the fibres; of *ivory* and *horn*, reduced firſt into thin tubes, and then cut in ſcrew; and of *quills*, by cutting alſo in ſcrew their barrels; I repeated, with thoſe inſtruments, my obſervations on *dew*; and to give a ſhort, but determinate idea of the phænomena I obſerved, I ſhall reduce them to ſome general caſes, as indicated by one only of thoſe *hygroſcopes*, that of *quill*, which, like all the others, is divided into 100 parts, from *extreme dryneſs* to *extreme moiſture*. Theſe hygroſcopes were ſuſpended in the open air, three feet above a graſs-plat in the country. 1ſt Case. When a clear and calm evening ſucceeds to a clear and warm day, the *graſs* frequently grows *wet*, though the above *hygroſcope* ſtands many hours, and ſometimes the whole night, between 50 and 55. 2d Case. If the *dew* increaſes, ſo that taller herbaceous *plants* and *ſhrubs* grow *wet* in ſucceſſion, the *hygroſcope* moves more and more towards *moiſture*; and when it is come to about 80, plates of *glaſs* and *oil-paint* alſo grow *wet*; but at that period, neither *metallic* plates, expoſed like the glaſs ones, nor ſome *ſhrubs* and *trees*, are *wet*; and this alſo may laſt whole nights. 3d Caſe. If the

dew

dew proceeds to its *maximum*, the *hygroſcope* moves from 80 to 100 (and ſometimes a little farther, § 27.). Then we have alſo a certain proof that *extreme moiſture* exiſts in the air; for every ſolid body expoſed to it is *wet*. But it is only at that moment that we can depend on *extreme moiſture* exiſting; for, if in the other deſcribed ſtages of the phænomenon, the appearance of *water* on the ſurface of ſome ſolids had proceeded from a ſpontaneous precipitation in the air, all the other ſolids ought to have been *wet*; but they only become *wet* in a certain ſucceſſion, and in the mean time the ſlip of *quill*, and all the other above-mentioned *hygroſcopes*, move more and more towards their point 100, in ſign of *moiſture* increaſing in the air. Conſequently (as I had concluded from my firſt obſervations), inſtead of having in *dew* an *hygroſcopic* ſtandard for the *hygrometer*, we have in its phænomena many circumſtances which will only be explained with the aſſiſtance of that inſtrument.

29. Some previous obſervations had alſo warned me againſt the general idea, that *moiſture* was to be *extreme* in the *air*, when there was a ſufficient quantity of *water* in the ſpace, even though that *air* might be ſuppoſed to be filled with *evaporated* water to its *maximum*; and the doubts I entertained in that reſpect were the cauſe of the difficulties I expreſſed in the beginning of my firſt Paper, which I only got over when I thought of *water* itſelf, to obtain the point of *extreme moiſture* on my *hygrometer*. This was alſo the reaſon why, as ſoon as my firſt *hygrometer* was finiſhed, I placed it in a cellar, the walls and ground of which were *wet*, and where it continued two months, without ever attaining its point of *extreme moiſture*. I have related that experiment in § 54. *et ſeq.* of my firſt Paper.

30. When alſo I had the *hygroſcopes* mentioned above in the obſervations on *dew*, I undertook a very long courſe of various ſorts of experiments on that important point of *hy-*

grology, of which however I ſhall only give here the general and conſtant reſults, as furniſhed by thoſe hygroſcopes. "The "*maximum* of *evaporation* in a maſs of incloſed *air* is far from "being identical with the *maximum* of *moiſture*; this being "dependent alſo, even to a very great degree, on the *tempera-* "*ture* of the *ſpace*, ſuppoſed to be the ſame, or nearly ſo, as that "of the *water* which *evaporates* in it. *Moiſture* may arrive to "its *extreme* in an incloſed *air*, if that common *temperature* "is near *freezing point*; but it becomes leſs and leſs, even to a "very *dry* ſtate, as that *temperature* riſes, though the *product* "*of evaporation*, thereby increaſing, continues to be at its dif- "ferent *maxima*, correſpondent to the different *temperatures*." This is a very important propoſition in *hygrology*; which, from my experiments, would not be ſubject to any objection, if there were no other *hygroſcopes* than thoſe I have mentioned above, of which I have thirteen different ſpecies; but there is another claſs of ſuch inſtruments, from which ſome doubt might firſt ariſe; and I come now to that point.

On two diſtinct claſſes of hygroſcopes.

31. As I ſhall now frequently ſpeak of *ſlips* and *threads*, which conſtitute thoſe two *claſſes* of *hygroſcopes*, I muſt firſt explain what I mean by thoſe words. The *ſlips* compoſe the claſs of *hygroſcopes* uſed in the above experiments; they conſiſt of very thin and narrow *laminæ* cut *acroſs* the fibres of vegetable or animal ſubſtances, either in their natural or artificial breadths (as *boards*), or by reducing natural or artificial thin tubes of them into *helices*. By *threads* I mean the ſame kinds of ſubſtances taken *lengthwiſe*, either from their being naturally in thin threads, or by reducing them to that ſtate, in

tearing from them thin *faſciculi* of *fibres*; which operation is eaſy in ſome, as *hemp*, *whalebone*, and *gut*, but very difficult in others, as *quill* and ſome ſorts of *wood*.

32. The firſt *hygroſcopes* of the claſs of *threads*, which I obſerved comparatively to the claſs of *ſlips*, were of *hemp*, *gut*, *whalebone*, and ſome *woods*, and they exhibited a phænomenon which at firſt I could not underſtand: when they were expoſed with the *ſlips* in *damp* air; as, for inſtance, in open air during the ſecond period of *dew* above determined, or in a glaſs veſſel inverted over *water*; the *threads* had only very ſmall motions backwards and forwards round their point determined in *water*, while the *ſlips* had conſiderable motions within that point, without coming near it, if the *temperature* was ſenſibly above *freezing point*. Thence ariſes the objection againſt the general propoſition above ſtated: for theſe two claſſes of *hygroſcopes* contradicting one another on the changes of *moiſture*, nothing could be aſſerted in that reſpect, till there were ſufficient reaſons to exclude one of thoſe claſſes of informers, and to truſt the other claſs.

33. Proceeding to multiply the ſpecies of thoſe two claſſes of *hygroſcopes*, I found always the ſame fundamental *march* in *ſlips*, all of them conſtantly moving in the ſame direction; but in multiplying the ſpecies of *threads*, I found ſuch variety between them, that in their own claſs they created diſtruſt; ſome of them, as of *deal*, *aloes pitta*, *liber of lime tree*, *quill*, and thin *ſtems of gramen*, in coming out of *water*, increaſed in *length*: they went farther that way, to a certain point, as the air was dryer: they *retrograded* then with accelerated ſteps, when *dryneſs* increaſed, thereby returning to the point where they had ſtood in *water*; and they continued to move in the ſame *retrograde* way, with great acceleration, by a ſtill increaſing

creaſing *dryneſs*. Moreover, they did not follow one another in thoſe motions contradictory to the evident *march* of *moiſture*: each of them changed direction at different periods, thereby often contradicting alſo each other, while the *ſlips* conſtantly agreed together in the direction of their motion, and alſo with all the other ſymptoms of *moiſture*. From theſe comparative phænomena I firſt concluded; that the motions of the ſame kind, which I had obſerved in the firſt-mentioned *threads*, were alſo *anomalies*, proceeding, only to a ſmaller degree, from a cauſe of the ſame nature as that of theſe laſt. After which, ſimilar ſymptoms, which I had formerly obſerved in the *water* thermometer near the *freezing point*, made me firſt conclude, from a general analogy; that the perceivable modifications of the *threads*, were the compound effects of two contrary operations of *moiſture* which followed different laws.

34. Another phænomenon led me ſoon after to a more determined theory in reſpect of thoſe two oppoſite effects of *moiſture* on *threads*. I have ſaid above, that *hemp* and *gut* have only a very little *retrogradation*; their greateſt difference from the *ſlips* conſiſting in their being *ſtationary*, while the *ſlips* have ſtill great motions. But when theſe ſame *threads* are *twiſted*, they acquire a very ſenſible *elongation* beyond their point of *extreme moiſture* ſucceeded by *retrogradation*. From ſeveral trials I have made in *twiſting* theſe *threads* more and more, I do not conſider as impoſſible, if ſome difficulties, which I only could obviate in part, were completely prevented, that they might be brought to ſuch a ſtate, as to have their point of *extreme* dryneſs coincide with that of *extreme moiſture*; by which means, in the progreſs of *moiſture* from one *extreme* to the other, they would move firſt in one direction with decreaſing ſteps, then in the oppoſite direction by increaſing ſteps; the whole, however,

with great irregularities. Here then we ſee two *oppoſite* effects of *moiſture*; one which *lengthens* the *fibres*; the other which, by *ſwelling* the *twiſted* ſtrings, *ſhortens* them; and we ſee thoſe effects follow different *laws*, from which is produced a *retrogradation* that we may change *ad libitum*.

35. Now, the texture of animal and vegetable *fibrous* ſubſtances muſt be a ſort of *reticle*, which exiſts in thoſe which are naturally in thin *threads*, and in the moſt minute *faſciculi* that we can ſeparate from a maſs; and we ſee it in the laſt caſe, for in ſubdividing thoſe *faſciculi*, there are always *fibres* breaking in the points where they were anaſtomoſed with others; conſequently, the primary *fibres* of thoſe ſubſtances form between them *meſhes* ſimilar to thoſe of a *net*; and thoſe *meſhes*, which are *widened* by the introduction of *water*, muſt produce in the *threads* the ſame effect as the *twiſt* in the above *ſtrings*.

36. If then *moiſture*, in acting on vegetable and animal *threads*, natural and artificial, produces on their *length* two oppoſite *effects*; one of which, ſmall at firſt but increaſing gradually, *compenſates* at ſome period the other which is firſt viſible, and *ſurpaſſes* it afterwards, ſooner or later, according to the nature of the *threads*; it is evident, that they cannot be proper for the *hygrometer*; ſince, from the indication of ſome of them it might ſometimes be concluded, that *moiſture* changes in one ſenſe, while it really changes in the contrary ſenſe; and from ſome others, that *moiſture* is *extreme*, long before it is really ſo. As for the *ſlips*; ſince *moiſture* has only one effect on their *length*, that of *widening* more or leſs the *meſhes* of the *croſs fibres*, I concluded; that all their *hygroſcopic* indications, in every part of their ſcale, were *true* in reſpect of *increaſe* and *decreaſe* of *moiſture*; and that conſequently, that claſs of *hygroſcopes* might be depended upon on that important point.

point. As for the exact *ratio* between the *indications* of thoſe laſt *hygroſcopes*, and the *changes* of *moiſture*, that was to be the object of a particular inquiry, to which I now come.

Of the ſcale *of the* hygrometer *between the two* fixed points.

37. The long attention I had formerly given to the comparative *expanſions* by *heat* of various kinds of *liquids* and *ſolids*, made me expect the variety I afterwards found in the modifications of *hygroſcopic* ſubſtances by *moiſture*; therefore, as I expreſſed it in § 2. of my firſt Paper, my view was only at that time, to find ſome means of producing a ſteady *comparable* hygrometer; but afterwards I pointed out, in § 72, a means which had occurred to me, for attempting to find the *ratio* between the *expanſions* of ſome determined *hygroſcopic* ſubſtance, and the correſpondent *increaſes* of *moiſture*; which was, to compare the firſt with the correſpondent changes of *weight*, of the ſame or of any other ſubſtance; an idea which I did not then much ſcrutinize, not yet thinking of its execution.

38. From what I have ſaid above, I did not want any other motive of choice between the *ſlips* and the *threads* than their comparative *marches*; but though the *ſlips* agree always in the *direction* of their motions, there are differences in the progreſſion of their comparative ſteps; and that difference led me to examine more attentively the above means of finding which of thoſe *marches* agreed beſt with that of *moiſture*. The reſult of that examination was diſtruſt, at leaſt in reſpect of an immediate deciſive means. With the view of rendering the changes of *weight* more eaſily meaſurable, I had firſt thought of ſome ſubſtance poſſeſſed of a ſtrong *affinity* with *water*; but on conſidering

ſidering the *hygroſcopic* phænomena of thoſe ſubſtances, it appeared to me, that their changes of *weight* could not be proportional to the degrees of *moiſture* in the *medium*; and that even the ſenſe of the word *moiſture* applied to them was very difficult to determine (§ 14.). As for the ſubſtance of the *hygrometer* itſelf, I did not find any reaſon to think, that its changes of *weight* could be more proportional to *moiſture* than its degrees of *expanſion*; ſince on theſe laſt depended in part the quantity of *water* that could be admitted into its pores at each degree of *moiſture* in the *air*.

39. Being thus diſappointed in my firſt ſcheme, I thought of a more direct method, which was to act on *moiſture* itſelf, by firſt producing, in a glaſs veſſel containing an *hygroſcope*, as much *dryneſs* as I could conceive poſſible at that time, and then introducing into it ſucceſſive equal quantities of *water*, for which I had found a ſure means without opening the veſſel. But then again ſome previous experiments deſtroyed my confidence in that method; having found,—1ſt, That the evaporated *water* had a tendency to depoſit itſelf againſt the *glaſs* by the ſmalleſt difference between the inſide and outſide *temperatures*, even to the degree of becoming viſible on ſome part of the veſſel, long before I had any reaſon to expect *extreme moiſture*.—2d, That by the common *temperature* of my room, the *hygroſcope* in the veſſel remained always at a conſiderable diſtance from its point of *extreme moiſture*, though the bottom of the veſſel was covered with *water*; and that it varied with the *temperature*; which could not have happened if *moiſture* had been *extreme*.—3d, That when I endeavoured to increaſe *moiſture* in the veſſel by cooling it, I produced very often the contrary effect, at the ſame time that a quantity of the diſſeminated *water* gathered over the glaſs.

40. As in those trials the contradiction between the *marches* of the *slips* and the *threads* was very evident, I was the more disappointed to find, that the uncertainty of the real degrees of *moisture* increased at that very period: for instance, setting out from some *dry* point, and *moisture* increasing, the *march* of a *thread* of *whalebone* was evidently in a *decreasing* progression, comparatively to that of a *slip* of the same substance; and when at last there was a superfluous quantity of *water* in the vessel, and the *temperature* was made to change, the *thread* was almost *stationary*, while the *slip* had considerable motions, often contrary to the small motions of the other; while it was at that very period, that the real degrees of *moisture* in the vessel could not have been ascertained but by an already settled *hygrometer*. I had no doubt that those anomalies were to be attributed to the *thread* of *whalebone*, not only because of the excess of the same modification in other *threads*; but also considering the analogy between the comparative *marches* of the two kinds of *hygroscopes*, and those of the *thermometers*, of *water* and *quickfilver*: but as this was a very important object for natural philosophy, I would not decide it from those first appearances; and that consideration led me to a very great number of various sorts of experiments, which I made in view of multiplying at least the indirect facts with which my theory might be compared. However, as at last I found a more direct means of verification, I shall only mention that last class of experiments.

Expe-

Experiments on the comparative changes of weight *and* dimenſions *of ſome* hygroſcopic ſubſtances.

41. I have ſaid above, that I could not find any ſolid reaſon to conſider the changes in *weight* of a ſubſtance, as being more proportional, than its changes in *dimenſions*, to the correſpondent changes of *moiſture* in the *medium*; which doubt had prevented me from undertaking that courſe of experiments. But it occurred to me at laſt, that if my theory on the comparative *marches* of the ſlips and the threads were true, it might be rendered certain by comparing thoſe marches with the increaſe of *weight* of the ſame ſubſtance: for inſtance, taking a *ſlip* and a *thread* of *deal*, and having ſome *deal* hung to a ſcale, I was to find, that while the *ſlip* continues to lengthen, and the thread to ſhorten, the ſubſtances continue to receive *water*; and, in general, that the *march* of *ſlips*, in every part of their ſcale on which the experiment may be regular, was more proportional, than that of *threads*, to the correſpondent changes in *weight* of every *hygroſcopic* ſubſtance of the *elaſtic* kind.

42. This having ſtruck me as a ſure means of deciding the queſtion, I ſet immediately to work; and ſince that time I have made a great number of experiments of that kind. They were not at firſt very accurate; but ſucceſſively I have mended both the inſtruments and the apparatus; and, after having ſettled every part that I looked upon as eſſential, and made in conſequence a new apparatus and new inſtruments, I have begun a regular courſe of experiments, of which I ſhall give here the firſt reſults.

43. The apparatus conſiſts in two tin veſſels; the firſt of which, and the moſt uſed, is 16¼ inches high, 15¼ wide, and 5 deep. The front of this veſſel is a plate of glaſs, and the back a tin-plate *ſlider*, which, being taken off, leaves that ſide

of the veſſel quite open. The ſecond veſſel has the ſame dimenſions as the firſt; but its back is ſoldered, and its front is of woven braſs wire. This veſſel may be applied to the back of the former, in ſuch manner as to make of both one ſingle veſſel, which, when the *ſlider* of the fore-part is taken off, is only divided by a vertical partition of woven braſs wire. The uſe of that ſecond veſſel is to produce *extreme dryneſs* in the other; for which purpoſe it is filled with large pieces of *quick-lime* taken from the kiln. When that veſſel is not uſed, it is kept in a tin box which it fills intirely; and when it is in, as well as while it is out for uſe, that box is kept ſhut with putty, by which means the ſame *lime* may ſerve many times in the following manner.

44. When I want to produce *extreme dryneſs* in the firſt veſſel, I apply to it the ſecond, faſtened with hooks; I then pull out the *ſlider* of the firſt, and ſtop with putty the chinks between them. When that firſt operation is completed, I put again the *ſlider* to the fore-veſſel, and take off the other. In this laſt operation, ſome *moiſture* might be introduced through the chinks of the ſlider, before they are again ſtopped with putty; eſpecially as the deſtruction of *moiſture* in the veſſel has made room for more *air* to come in; but I prevent it by making firſt the apparatus ſenſibly warmer than it was when I put on the lime-veſſel; by which means, in the little time employed for the operation, the motion of the air is from the inſide to the outſide, which prevents all acceſs of *moiſture* in the veſſel.

45. To that firſt operation ſucceeds that of a gradual introduction of *moiſture* into the apparatus. It would have been uſeleſs to apply to that proceſs the means I had imagined, for ſpreading ſucceſſive equal quantities of water in a cloſe veſſel; for the evaporated *water* depoſiting itſelf in part, more or leſs, on every ſurface, and the ſurfaces, eſpecially of *glaſs*, being very

very much multiplied in that apparatus when filled with inſtruments, no real correſpondence could be expected between the quantity of evaporated *water* and the motion of the inſtruments; therefore I again gave up the view of determining that *ratio*, and proceeded in the following manner. At the bottom of the apparatus, on one of its ſides, there is an opening, ½ an inch high, and 2½ inches broad, which uſually is kept ſhut with a tin plate and putty. The taking off, only for a moment, that tin plate, is the firſt operation by which I introduce *moiſture* into the apparatus; which being then reduced to the *temperature* at which I make all my obſervations, namely, 60° of FAHRENHEIT, permits the external air to enter the veſſel, and come to an *equilibrium* with it at the ſame temperature. By keeping off the tin plate longer and longer, I admit new quantities of *moiſture* into the veſſel; and when that means is become ineffectual, I introduce through the ſame opening, a braſs frame, which extends under all the inſtruments, on which is ſtretched a cloth that I *wet* by degrees more and more, as long as it may produce ſome effect at the above *temperature*. In order to have that *temperature* when I want it, I make thoſe experiments in a ſeaſon when I may have fire in a ſtove at a proper diſtance from the apparatus. The time when that equal *temperature* is neceſſary in many reſpects, is that of the obſervations, which I make twelve hours after each new introduction of *moiſture*.

46. The inſtruments I place in that apparatus are of two kinds; the firſt of which are *beams*, made on the principle uſed by Mr. JOHN COVENTRY in his *paper hygrometer*, which principle I have found of great uſe; for, with *beams* of that ſort, as delicate as mine are, if the total change of weight in an experiment is not above 1 grain, $\frac{1}{1000}$ part of it may be diſtinctly obſerved on a weight from 3 or 4 to 20 grains; but in my experiments,

in which the total variation was from 5 to 6 grains, the observable part was only $\frac{1}{160}$ part of a grain. These two *beams* are placed on the same line through the middle of the depth of the vessel, and their indices move in that plane; their motions are in an opposite sense by the same changes of weight, because I wanted the two substances suspended to the *beams*, to hang near one another in the middle of the vessel. The other instruments are *frames*, in which an *index* is moved by the variations in *length* of a very thin *slip* or *thread*. These frames are placed before and behind the *beams*.

47. In the first experiment that I made with that apparatus, the substances suspended to the *beams* were *deal* and *quill*, reduced to very thin *shavings*, which were stretched edgewise in thin brass-wire frames. The weight of each kind of these *shavings* was 12 grains, at a certain degree of the thermometer and of my hygrometer. The other *hygroscopes* were *slips* and *threads* of the same substances as the *shavings*, and also of *whalebone*. These six last instruments have their point of *extreme dryness* taken in my *lime-vessel*, and their point of *extreme moisture* in *water*; the interval between these points is divided into 100 parts; and on the *scales* of the *threads* the *degrees* are prolonged beyond this last point. The *hygroscopic* scale of the *shavings* could not be fixed before the operation; therefore the scales of the beams served only to indicate the comparative motions of the index; but afterwards, taking for 0, the point where the *index* stood by *extreme dryness*, and for 100, the point of *extreme moisture*, which I shall explain, the interval between these two points became a *modulum* by which I have also reduced into 100 parts of the *whole* the changes observed in the *weight* of the *shavings*. I shall not give here the absolute quantities, either of the changes of *weight*, or

of

of thoſe in *length* of the other *hygroſcopes*, having not had time to make the neceſſary calculations, of which however I have the *data*.

48. The neceſſary time for a complete diffuſion of the newly introduced *moiſture* in the veſſel renders it impoſſible to proceed, in that introduction, by regular ſteps. The method I uſe is, to obſerve the motion of my uſual *hygrometer*, which is a *ſlip* of *whalebone*, and to remove the cauſe of increaſe of *moiſture* before it has moved 5 degrees. In that manner the ſteps of the increaſing *moiſture* have been in general leſs than 5 degrees of that inſtrument; but, by interpolation, I have reduced them to what they would have been if the ſame inſtrument had been moved ſucceſſively 5 degrees.

49. I have ſaid before, that when the *maximum* of *evaporation* is produced in a cloſe veſſel by a *temperature* ſenſibly above the *freezing point*, there is no regularity to be expected in any farther attempt to increaſe *moiſture*; the diſſeminated *water* being then abundant, the ſmalleſt difference of temperature between different parts of the apparatus makes it depoſit itſelf on ſome ſurface, and paſs from one to the other (§ 39.); which circumſtance is alſo mentioned by Profeſſor PICTET of Geneva, in his late work, *Eſſais de Phyſique*. For that reaſon, when the *evaporated water* in the veſſel was near its *maximum*, my laſt operation was, to put in again the *wet* cloth, while I kept the *temperature* at 60°, and to take it out when the *indices* of the *beams* were fixed. My uſual *hygrometer* was then at 87°; and as it had ſtill 13 *degrees* to move towards its point of *extreme moiſture*, and all the others in proportion of their known *marches*, I have added to the obſerved increaſes of *weight* in the *ſhavings*, a quantity proportional to their former marches

comparatively to that of their reſpective *ſlips*; and thus are completed their *hygroſcopic ſcales*. Repeated trials have ſhewn me, that the *weight* of the *ſhavings* increaſes as long as their *ſlips* increaſe in *length*; but as there is no regularity in their comparative *marches* at that period of *moiſture* in a veſſel, nor any poſſibility of making theſe experiments in open air, becauſe of the *beams*; the addition mentioned, which forms the three laſt *terms* of the columns of the *ſhavings* in the following table, is to be conſidered only as having determined the *modulum* of the *obſerved* terms; ſince it has not changed the *ratio* between them, nor conſequently the correſpondent *marches* of *weight* and *length* ſo far; which were the only object of the experiment.

50. Before I come to the general reſult of that experiment, I ſhall place here a comparative view of the two kinds of phænomena, which, by their analogy, led me firſt to the theory I have expoſed: I mean the comparative *marches* of the *ſlip* and *thread* of *whalebone* on one ſide, and thoſe of the *thermoſcopes* of *quickſilver* and *water* on the other ſide. In this table the correſpondent *terms* of the *hygroſcopes*, from 0 of both to 85 of the *ſlip*, have been obſerved in the above experiment; the four following are the reſults of obſervations in time of increaſing *dew*. The correſpondent *terms* of the *thermoſcopes* are deduced from the *table* of their comparative *expanſions* which I have given in § 418. *m.* of my work, *Rech. ſur les Mod. de l'Atmoſphère*; from which *table* this only differs, 1ſt, by a change of the *modulum*, in the *ratio* of 80 to 100; 2d, by an *inverſion*, which brings the *terms* of this table to expreſs comparative *condenſations* of the two *liquids*.

HYGRO-

Hygroscopes.			Thermoscopes.		
Whalebone Slip.	Whalebone Thread.		Quickſilver.	Water.	
Extreme 0	0,0	*dryneſs.*	*Boiling* 0	0,0	*point.*
5	12,1		5	9,3	
10	30,1		10	18,3	
15	41,1		15	26,3	
20	51,1		20	35,0	
25	59,1		25	42,7	
30	65,6		30	49,2	
35	71,1		35	56,7	
40	76,5		40	63,1	
45	81,8		45	69,0	
50	85,8		50	74,5	
55	88,8		55	79,1	
60	91,3		60	83,8	
65	93,3		65	87,9	
70	95,6		70	91,8	
75	97,6		75	95,0	
80	98,6		80	97,5	
85	99,6		85	98,9	
90	100,1		90	99,9	
95	100,5		95	100,5	
Extreme 100	100.	*moiſture.*	*Freezing* 100	100.	*point.*

51. The firſt part of that table ſhews, the great ſteps of the *thread* of *whalebone*, comparatively with thoſe of the *ſlip* of the ſame ſubſtance, at the beginning of their correſpondent increaſe in *length*; but the *thread* relents by degrees, and a nearly *ſtationary* ſtate ſucceeds, in which, while this *hygroſcope* moves firſt 1,9 *degree* from 98,6 to 100,5, then *retrogrades* 0,5

degree to come to its point of *extreme moiſture*; the *ſlip*, continuing to move in the former direction, goes over 20 *degrees*. The phænomena are the ſame in the part of the table relating to the *thermoſcopes*; that of *water* proceeds alſo at firſt by great ſteps, comparatively to that of *quickſilver*; after which it relents, and while it moves only 3 degrees, and *retrogrades* 0,5 degree to come to the *freezing point*, the *quickſilver* one, continuing to move in the ſame direction, goes over 20 degrees. It was from that phænomenon of the *water thermoſcope* that I concluded formerly, that there was in its *march* a *ſtationary* ſtate, during which the *beat* decreaſed nearly as indicated by the *quickſilver* one; and that concluſion was afterwards confirmed by direct experiments. From that aſcertained fact, I was led to conclude, with reſpect to moiſture, that there was alſo a *ſtationary* ſtate in the *march* of hygroſcopic *threads*, even in thoſe which had the ſmalleſt *retrogradation*, as that of *whalebone*; and this theory will be confirmed by the reſults of the above-deſcribed experiment.

52. The following table contains thoſe reſults, namely, the correſpondent *marches* of all the mentioned *hygroſcopes*; the *ſhavings* increaſing in *weight*, and the *ſlips* and *threads* in *length*. The 3 laſt comparative *terms* do not reſult from that particular experiment; for the *ſhavings* (as I have ſaid above) they are concluded from the former comparative ſteps; for the other inſtruments, they have been obtained by obſervations in the open *damp* air.

WHALE-

	Whalebone.		Quill.			Deal.		
	Slip.	Thread.	Shavings.	Slip.	Thread.	Shavings.	Slip.	Thr.
Extr. dryn.	0	0,0	0,0	0,0	0,0	0,0	0,0	0,0
	5	12,1	7,0	4,8	40,0	6,2	5,4	42,0
	10	30,1	13,0	9,7	72,0	9,4	11,2	69,4
	15	41,1	20,0	14,4	85,0	15,6	16,5	94,8
	20	51,1	26,0	19,2	95,0	22,6	21,9	107,0
	25	59,1	31,0	23,9	101,0	27,0	27,2	113,6
	30	65,6	36,0	28,5	105,0	33,2	32,7	118,6
	35	71,1	42,0	33,3	107,0	36,0	38,3	122,6
	40	76,5	43,8	38,3	102,0	41,2	43,7	120,6
	45	81,8	48,3	42,9	104,0	46,7	49,2	123,6
	50	85,8	52,3	47,4	107,0	49,7	54,6	126,6
	55	88,8	56,5	52,4	103,0	56,1	59,9	119,7
	60	91,3	60,5	56,9	105,0	59,9	64,9	122,7
	65	93,3	64,4	61,9	106,0	63,7	69,7	119,7
	70	95,6	69,4	67,2	108,0	67,1	74,5	117,6
	75	97,6	74,0	72,2	107,0	73,4	79,0	115,6
	80	98,6	78,0	77,8	106,0	78,1	83,5	112,6
	85	99,6	84,0	82,8	105,0	83,8	87,5	110,0
	90	100,1	* 88,0	88,2	103,6	* 88,8	92,0	107,0
	95	100,5	* 94,0	94,0	102,0	* 93,8	96,0	103,6
Extr. moist.	100	100.	*100.	100.	100.	*100.	100.	100.

From the 18 first *terms* of that table, which are the immediate results of the experiment, we are now to examine my opinion, that the *lengthening* of the *slips* of *whalebone*, *quill*, and *deal*, beyond these *terms* is a sure sign that, till they have attained their point 100, *moisture* continues to increase in the *medium* where they are placed.

53. There could not be any doubt on that proposition, if it were not for some *threads* similar to that of *whalebone*; which *threads*, having sensibly attained their utmost *length* at the period when the experiment was stopped, seem to indicate, that

moiſture is then at its *maximum*. But if the *lengthening* of *hygroſcopic threads* in general, is the compound effect of two oppoſite cauſes which follow different laws; it may be that, in ſome *threads*, thoſe cauſes happen to compenſate each other at that very period; by which means they are *ſtationary*, though *moiſture* continues to increaſe. This was my opinion, and the above experiments were undertaken to verify it, firſt, by comparing the *march* of the *ſlips* with the increaſe in *weight* of their own ſubſtance; ſecondly, by comparing the *marches* of different kinds of *threads* with each other, and alſo with the increaſe in weight of their ſubſtance: and from that now we are to examine the above propoſition.

54. In reſpect of the *ſlips* my theory is, that as *moiſture* cannot act on their length but by widening the *meſhes* of their tranſverſal *fibres*, they cannot go on *lengthening* but by imbibing more and more *moiſture*, from its increaſe in the air; and this we ſee to be the caſe, by comparing the *marches* of the three kinds of *ſlips* with the correſpondent increaſes in *weight* of the *quill* and *deal* ſhavings, during the whole progreſs of the experiment. There are differences in thoſe *marches* as I expected (§ 37.); but they are not ſuch as to give the ſmalleſt reaſon to ſuſpect, that afterwards, during the period of the three laſt *terms* of the table, in which we have no correſpondent obſervations of increaſes of *weight* in the *ſhavings* of *deal* and *quill*, the ſame law does not take place as in the 17 antecedent *terms*. If the experiment was only made with one kind of *ſlip*, it might be objected, that though that *ſlip* lengthens regularly during the whole increaſe of *moiſture* from its *minimum* to its ſuppoſed *maximum*, it is not impoſſible but that immediately after, by ſome peculiarity of its nature, it will *lengthen*, without any farther increaſe of *moiſture* in the *medium*.

medium. But that ſurmiſe cannot be admitted when the *ſlips* of ſuch diſſimilar ſubſtances as *whalebone*, *quill*, and *deal*, ſenſibly agree in their *motions* at that period, and when a number of other *ſlips* of the vegetable and animal kinds follow alſo the ſame general *march.*

55. In reſpect of the *threads*, which are the only cauſe of the above doubt, my theory, which removes it, is alſo confirmed by that experiment. That cauſe of doubt is exemplified in the table by the *thread* of *whalebone*, which has almoſt no motion, while its *ſlip* moves from 85 to 100. At that period of *moiſture*, no regularity can be expected from experiments made in *cloſe veſſels*; by which circumſtance, not having the correſpondent obſervations of *weights*, it cannot be demonſtrated by immediate experiments, that the *thread* of *whalebone* is then *ſtationary* from its nature; but the *threads* of *quill* and *deal*, which are in that ſtate during the regular courſe of the experiment, will guide us in that enquiry. The *thread* of *quill* is *ſtationary* during that great part of the obſerved increaſe of *moiſture*, by which the *thread* of *whalebone* moves from 71 to 97,6: the *thread* of *deal* is alſo *ſtationary*, while the ſame *thread* moves from 71 to 91,3. They both afterwards *retrograde*; the *quill* from 107 to 100, and the *deal* from 122,6 alſo to 100; and it is during the latter part of that *retrogradation*, correſpondent to a continued *direct* motion of the *ſlips*, that the *thread* of *whalebone*, and ſome more of its claſs, after a very *decreaſing* march comparatively with all the *ſlips*, are at laſt *ſtationary*, and then a little *retrograde*. In that *ſtationary* ſtate of the *threads*, while moiſture proceeds in the ſame direction, they move backwards and forwards, more or leſs, according to the duration of that ſtate, and to the quantity of the *retrogradation.* This may be ſeen by the table, in

reſpect

reſpect of the *thread* of *deal* and *quill*; and I have obſerved it conſtantly, in a ſmaller proportion, in all the *threads* which have their *ſtationary* ſtate at the laſt period of *moiſture*, with this particular circumſtance in all thoſe motions backwards and forwards, that they are never the ſame in two different experiments. That ſymptom already points out a complication of cauſes; but we ſhall ſoon ſee a more diſtinct proof of their exiſtence.

56. My theory on the *march* of hygroſcopic *threads* is founded on this general principle, that a *retrograde* effect, how ſmall ſoever it may be, if it is not produced by a correſpondent change in the cauſe itſelf, is preceded by a *ſtationary* ſtate, during which, and the *retrogradation*, the intenſity of the cauſe continues to *increaſe*: and this alſo is exemplified in the experiment. The *ſtationary* ſtate of the *thread* of *deal* begins, when its *ſhavings* have only imbibed from the air a quantity of *water* = 36; it is ſtill at the ſame point when the quantity of imbibed *water* has increaſed to 59,9; and in the part of its *retrogradation*, which is ſtill contained in the regular courſe of the experiment, that quantity of imbibed *water* increaſes to 88,8. The ſame, with only ſome differences in the degree, is ſeen in the *thread* of *quill*; therefore, as we ſee alſo ſome, though a very ſmall, *retrogradation* in the *thread* of *whalebone*, as well as in other *threads* of the ſame *claſs*, we have reaſon to conclude, that their apparent immobility before they come to that point, while the *ſlips* continue to move, is alſo a *ſtationary* ſtate, during which they continue to receive water, by the increaſe of *moiſture* in the air.

57. The experiments I have now analyſed are only one ſet amongſt others which, though made with leſs accuracy, have given the ſame general reſults. Theſe, relating to various

kinds of ſubſtances, I intend to repeat, and to have the honour of communicating their reſults to the Royal Society; and I ſhall conclude this Paper, with an immediate demonſtration, that the *hygroſcopic* motions of the *ſlips* are ſimple, while thoſe of the *threads* are the combined effects of two oppoſite cauſes: which will be a further confirmation of the whole of the above theory.

On the recoil *of hygroſcopic* threads.

58. When formerly I concluded from the phænomena of the *water thermoſcope*, that its *condenſations* were the combined effects of two oppoſite cauſes, which followed different laws, it was not for having diſtinguiſhed thoſe two effects; but only becauſe of a ſmall *retrogradation* near the *freezing point*, preceded by a *ſtationary* ſtate, comparatively with the *march* of *quickſilver*; but in the caſe of hygroſcopic *threads* and *ſlips*, in which we have the ſame phænomenon, the two oppoſite effects are diſtinguiſhable in the *threads*, by one being operated more rapidly than the other. If, for inſtance, I tranſport from a drier to a damper place (or inverſely) the two kinds of *quill* hygroſcopes, the *ſlip* proceeds in an *even* courſe to a certain point, where it remains *fixed*; but the *thread* moves in an *interrupted* manner alſo to a certain point, whence it *recoils*. If that experiment is made within the limits of the *ſtationary* ſtate of the *thread*, it may *recoil* as much as it has gone the other way, and be fixed at the ſame point in both places. The caſe of the *ſlip* of *quill* is common to every *ſlip*, and that of its *thread* to all others which have a quick motion. Here then we have *ſeparately* the *two* effects of *moiſture* on the *threads*; that on the *fibres* themſelves is the *ſooneſt* produced, and at firſt predominates: the *ſloweſt*, by

by which afterwards the firſt produced is more or leſs *compenſated*, is that operated on the width of the *meſhes*; and it is becauſe the laſt of thoſe effects is the only one that can affect the *length* of the *ſlips*, that, in every change of *moiſture*, they move *evenly*, without any *recoil*.

59. To that demonſtration of the exiſtence of *two* oppoſite *effects* of *moiſture* in the *threads*, I ſhall now only add an example of a ſimilar phænomenon, in which the *cauſes* alſo are viſible. The *compound frames*, mentioned in § 24. are formed of two *glaſs* rods 4 feet long, fixed together at the bottom and the top. A thin ſlip of *braſs* of determined length, fixed to one of the *glaſs* rods towards the top, comes down from thence, paſſes over a pulley at the bottom, and turns up for half an inch. To this end of the *braſs* ſlip is fixed the lower end of the hygroſcopic ſubſtance, the upper end of which is connected with the *index* of the inſtrument. It is by that means that, whatever be the changes of *heat*, provided they are *ſlow*, the lower end of the hygroſcopic ſubſtance remains ſenſibly at the ſame diſtance from the axis. But if I take that inſtrument out of water at a low *temperature*, and plunge it immediately into *warmer* water, the *index* inſtantly moves as if the hygroſcopic ſubſtance had lengthened; which is the *effect* of the *braſs* ſlip dilating ſooner than the *glaſs* rods; then the index *recoils*, and this is the *effect* of a *ſlower* dilatation of the *glaſs*.

Concluſion.

60. I have concentrated in theſe pages an account of twenty years aſſiduous labour in *hygrometry*, moſtly occaſioned by the anomalies of the *hygroſcopic threads*; and the principal reſults have been, ſome determinations of the four principles that directed

directed me from the beginning, which now are as follows. 1st, *Fire*, as cauſe of *heat*, is a ſure, and the only ſure, means of obtaining *extreme dryneſs*: this is produced by *white heat* in every *hygroſcopic* ſubſtance that can bear it; and it may be thus tranſmitted to the *hygrometer*. 2d, *Water*, in its liquid ſtate, is a ſure, and the only ſure, means of determining the point of *extreme moiſture* on that inſtrument. 3d, It is not to be expected, *à priori*, of any *hygroſcopic* ſubſtance, that its *changes* be proportional to thoſe of *moiſture*; but it may be affirmed, that no *fibrous* or *vaſcular* ſubſtance, taken *lengthwiſe*, is proper for the *hygrometer*. 4th, A means of throwing light on the *march* of a choſen *hygrometer*, may be, to compare it with the correſpondent changes in *weight* of many *hygroſcopic* ſubſtances.

61. From thoſe determinations in *hygrometry* ſome great points are already attained in *hygrology*, *meteorology*, and *chemiſtry*, of which I ſhall only indicate the moſt important. 1ſt, In the phænomenon of *dew*, the *graſs* often begins to be *wet* when the *air*, a little above it, is ſtill in a middle ſtate of *moiſture*; and *extreme moiſture* is only certain in that *air*, when every ſolid expoſed to it is *wet* (§ 28.). 2d, The *maximum* of *evaporation* in a cloſe ſpace, is far from identical with the *maximum* of *moiſture*; this depending conſiderably, though with the conſtant exiſtence of the other, on the *temperature* common to the *ſpace* and to the *water* that evaporates (§ 30.). 3d, The caſe of *extreme moiſture* exiſting in the open tranſparent air, in the day, even in time of *rain*, is extremely rare: I have obſerved it only once, the *temperature* being 39°. 4th, The *air* is *dryer* and *dryer* as we aſcend in the atmoſphere; ſo that in the upper attainable regions, it is conſtantly very *dry*, except in the *clouds*. This is a fact certified by M. DE SAUSSURE's obſervations and mine. 5th, If the whole atmoſphere paſſed

from *extreme dryneſs* to *extreme moiſture*, the quantity of *water* thus *evaporated* would not raiſe the *barometer* as much as half an inch. 6th, Laſtly, in chemical operations on *airs*, the greateſt quantity of *evaporated water* that may be ſuppoſed in them at the common *temperature* of the atmoſphere, even if they were at *extreme moiſture*, is not ſo much as $\frac{1}{100}$ part of their maſs. Theſe two laſt very important propoſitions, have been demonſtrated by M. DE SAUSSURE.

Fig. 1.

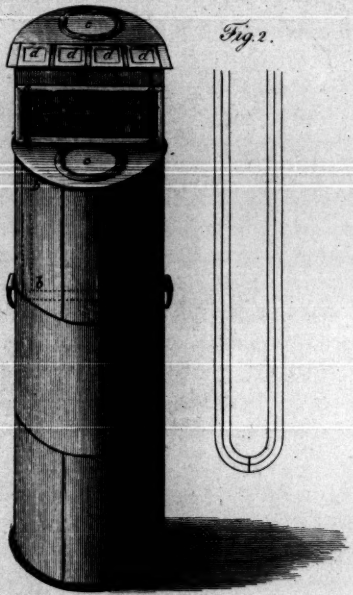


Fig. 2.

Bajire Sc.

II. *On the Production of Ambergris. A Communication from the Committee of Council appointed for the Conſideration of all Matters relating to Trade and Foreign Plantations; with a prefatory Letter from* William Fawkener, *Eſq. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 20, 1791.

TO SIR JOSEPH BANKS, BART.

Office of Committee of Privy Council for Trade, Whitehall, 15th January, 1791.

SIR,

LORD HAWKESBURY, Preſident of the Committee of Privy Council appointed for the Conſideration of all Matters relating to Trade and Foreign Plantations, having received a Letter from Mr. CHAMPION, a principal Merchant concerned in the Southern Whale Fiſhery, informing him, that a ſhip belonging to him had lately arrived from the ſaid fiſhery, which had brought home 362 ounces of ambergris, found by Mr. COFFIN, captain of the ſaid ſhip, in the body of a female ſpermaceti whale, taken on the Coaſt of Guinea; his Lordſhip thought fit to deſire Captain COFFIN, as well as Mr. CHAMPION, to attend the Lords of the Committee, that they might be examined concerning all the circumſtances of the fact before mentioned; and I am directed by their Lordſhips to tranſmit

to you a copy of the examination of these two gentlemen, that you may communicate the same to the Royal Society, if you should think that any of the circumstances, stated in this examination, will contribute to remove the doubts hitherto entertained concerning the natural history and production of this valuable drug. I send you also a piece of the ambergris so taken out of the whale, and some of the bills of the fish called Squids (which are supposed to be the food of spermaceti whales), and which were found partly in the ambergris taken from this female whale, and partly on the outside of it, and adhering to it.

I have the honour to be, &c.

W. FAWKENER.

At the Council Chamber, Whitehall, the 12th January, 1791.

By the right honourable the Lords of the Committee of Council appointed for the Consideration of all Matters relating to Trade and Foreign Plantations.

READ—Letter from Mr. ALEXANDER CHAMPION, a principal merchant concerned in the Southern Whale Fishery, to Lord HAWKESBURY, dated the 2d instant, acquainting his Lordship, that Captain JOSHUA COFFIN, of the ship The Lord Hawkesbury, is lately arrived from the Southern Whale Fishery; and that the said ship, besides a cargo of 76 tons of spemaceti oil and head-matter, has brought home about 360 ounces

ounces of ambergris, which the ſaid captain took out of the body of a female ſpermaceti whale on the Coaſt of Guinea.

Meſſ. CHAMPION and COFFIN attending, were then called in, and the following queſtions were put to Mr. COFFIN, *viz.*

Q. Have any of the whales, taken before by ſhips ſailing from Great-Britain, to your knowledge, contained any ambergris?

A. None, that ever I heard of. The American ſhips have, at times, found ſome.

Q. Was the ambergris, found by you, in a bull or cow fiſh?

A. It was found in a cow fiſh.

Q. Is it uſual to look for ambergris in whales that are killed?

A. It has not hitherto been much the practice to do ſo.

Q. How happened it that you diſcovered this?

A. We ſaw it come out of the fundament of the whale; as we were cutting the blubber, a piece of it ſwam upon the ſurface of the ſea.

Q. In what part of the whale did you find the remainder?

A. Some more was in the ſame paſſage, and the reſt was contained in a bag a little below the paſſage, and communicating with it.

Q. Did the whale appear to be in health?

A. No; ſhe did not. She ſeemed ſickly, had no fleſh upon her bones, and was very old, as appears by the teeth, two of which I have. Though ſhe was about thirty-five feet long, ſhe did not produce above one ton and a half of oil. A fiſh of the ſame ſize, in good health, would have produced two tons and a half.

Q.

Q. Have you obſerved the food that whales generally feed upon?

A. The ſpermaceti whale feeds, as I believe, almoſt wholly upon a fiſh called Squids. I have often ſeen a whale, when dying, bring up a quantity of ſquid, ſometimes whole, and ſometimes pieces of it. The bills of the ſquid (ſome of which Mr. COFFIN produced) were found, ſome in the inſide, and ſome on the outſide of the ambergris, ſticking to it.

Q. Did you ever find any ambergris floating on the ſea?

A. I never did, but others frequently have.

Q. How long have you been engaged in the whale fiſhery?

A. It is about ſixteen years ſince I firſt entered into it.

Q. What is the general proportion of bull and cow whales you have met with?

A. I believe the proportion to be nearly equal. In my laſt voyage, however, I found only four bulls out of thirty-five whales. I fiſhed upon the Coaſt of Africa between five North and ſeven South degrees of Latitude. I am inclined to think, that the cow whale goes to calve in the low latitudes, which accounts for more cows being found in thoſe latitudes.

Q. Is there any particular ſeaſon when the cow whales calve?

A. I do not know that there is.

Q. Does the bull or cow whale, in proportion to their ſize, produce moſt oil?

A. The cow whale, when big with calf, produces more oil than a bull whale of the ſame ſize; when ſuckling, ſhe produces leſs.

Q. Are the whales uſually found ſingly, or in pairs? or in larger numbers?

A. Uſually in large numbers, which we call Scools, and particularly in the low latitudes. I have ſeen from fifteen to perhaps a thouſand together.

Q. Have you any further information on this ſubject to give the Committee?

A. We have generally obſerved, that the ſpermaceti whale, when ſtruck, voids her excrement; if ſhe does not, we conjecture that ſhe has ambergris in her. I think ambergris moſt likely to be found in a ſickly fiſh; for I conſider it to be the cauſe or the effect of ſome diſorder.

Queſtions put to Mr. CHAMPION.

Q. At what price does ambergris uſually ſell; and at what price did that, taken by your ſhip, ſell?

A. A ſmall quantity had lately ſold at 25*s. per* ounce; but it was then very ſcarce. Mine ſold for 19*s.* 9*d. per* ounce. The whole quantity, found in this whale, was 362 ounces Troy. The people who bought it told me, this was a larger quantity than was ever before brought at once to market. It has been generally ſold at about 4 or 5 pounds at a time.

Q. For the uſe of what country was this ambergris bought?

A. I do not exactly know. It was bought by a broker, who told me, that his principal, who purchaſed about one half, bought it for exportation to Turkey, Germany, and France. The other half was purchaſed by the druggiſts in town.

III. *Observations on the Affinity between Baſaltes and Granite.* *By* Thomas Beddoes, *M. D.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 27, 1791.

ALL our opinions on the formation of rocks and mountains, except volcanic mountains, muſt of neceſſity reſt upon analogical reaſoning, ſince we have no direct teſtimony concerning their origin. Hence, whatever portion of the mineral kingdom is but little connected with our experience of the action of fire or water, muſt be ſlightly paſſed over, or ſet aſide for future inveſtigation, while the partizans of the two oppoſite hypotheſes, which at preſent divide the philoſophical obſervers of foſſils, fix their whole attention, and lay all the ſtreſs of their arguments, on ſuch particulars as they are able to connect by ſome analogy with the chemical operations in which either fire or water are principally concerned. For this reaſon, baſaltes has been much more the ſubject of diſputation than granite; the former ſpecies of rock offering appearances that coincide in ſome degree with both kinds of chemical proceſſes, while the latter ſeems to ſtand aloof from the experiments that have given birth to our ſciences. We do, indeed, find opinions on the production of granite by one or other of the cauſes above mentioned; but they are generally * looſe conjectures, thrown out at random, rather than philoſophical propoſitions, laid down in preciſe

* The only exceptions known to me are, Phil. Tranſ. Vol. LXV. p. 5—47. and Edinburgh Tranſactions, Vol. I. p. 255—257.

terms, and supported by proper evidence. In consequence of information obtained from various sources, I have been led to consider this question in a light somewhat new; and although I cannot but expect my conclusions to be controverted, however strong may be my own conviction of their justness, I am encouraged by the hope of communicating some original observations on the physical geography of our own country, and of bringing forward to public notice some facts not generally known among us, though they are such as cannot be overlooked in a theory of granite.

Notwithstanding the recent objections of Mr. WERNER *, I shall assume the origin of basaltes from subterraneous fusion as thoroughly established by various authors †. Several observations of my own, which I intend soon to offer to the publick, will, I flatter myself, corroborate the evidence, though already sufficiently strong to remove all reasonable doubt, and add a considerable tract to those where the effects of ancient fire have

* Bergmännisches Journal, March 1789. Among the facts adduced by this celebrated mineralogist to prove the watery origin of basaltes, I cannot discover any very new or striking. The appearance of basaltes between strata is his great argument. But the same, or a similar appearance had been noticed by Dr. HUTTON (Edinb. Tr. I. p. 279.); WHITEHURST (Chap. XVI.): FERBER (Italy, p. 51.) mentions the insertion of lava between calcareous strata; and in LESKE (Reise durch Sachsen, 4to, 1785, p. 517.), we have basaltic columns in sandstone. Mr. WERNER also insists upon the gradual approximation of the adjacent stratum towards the nature of basaltes.

† DESMAREST, in his well-known Paper in the French Memoirs; RASPE, Extinct Volcanos near Cassel; FAUJAS, Volcans éteints, &c. and Minéralogie des Volcans; Sir W. HAMILTON, Campi Phlegræi, and Phil. Trans.; FERBER, Travels through Italy; DOLOMIEU, in his Account of the Lipari and Pontian Isles; LESKE, *l. c.*; BERTOLDINGEN, in his Account of the Quicksilver Mines in the Palatinate, &c.; HAMILTON, Letters on the Coast of Antrim; HUTTON, *l. c.*; VELTHEIM, Etwas über die Basalt, Leipsig, 1787; are among the chief of these authors.

been traced in our times. It may be proper to premiſe, that under the term baſaltes I comprehend that vaſt natural family of rocks which is frequently cracked into regular colonnades, and may be followed in an unbroken ſeries from this perfect form through endleſs modifications to the moſt ſhapeleſs maſs of trapp or whinſtone. Though frequently of an iron-grey colour and uniform texture, this ſpecies of ſtone varies greatly in both theſe characters, even in the ſame rock. In particular, it paſſes, by the moſt inſenſible gradations, both to the porphyries, with which it coincides in appearance, in compoſition, and doubtleſs alſo in origin, and to the *hornſtein* of the Germans; a term including petroſilex and ſeveral ſorts of cloſe grained whinſtone, of which I have found in England varieties with a conchoidal fracture, ſemi-tranſparent at the edges, and in other reſpects faſt approaching * to a ſiliceous nature. Lilleſhall Hill near Shifnal, in Shropſhire, to mention a ſingle inſtance, affords ſuch ſiliceous, beſides *ſemi-granitic*, porphyritic, and common whinſtone, containing agate.

But baſaltes, of which a right knowledge is conducting us faſt to a juſt theory of the earth, is not leſs connected with granite; *inſomuch that we may trace theſe rocks gradually approaching and changing into one another*. I have myſelf had an opportunity of examining many connecting links in this gradual ſucceſſion; and this opinion, which has ſince been confirmed by other conſiderations, was firſt forced upon me by ſpecimens in great variety from volcanic and baſaltic countries. But as it is a point by far too important to be admitted on the mere authority of any mineralogiſt, I ſhall endeavour to ſupport it by the teſtimony of obſervers, who cannot be ſuſpected

* DOLOMIEU, Iſles Ponces, p. 111. 185. &c. deſcribes juſt ſuch lavas.

of any bias towards ſuch an hypotheſis. The firſt ſtep in the progreſſion appears at the Giant's Cauſeway in Ireland. Many of the pillars there conſiſt of fine-grained, dark-coloured whinſtone; that variety which may be conſidered as moſt perfect, and as equidiſtant from porphyry, petroſilex, and granite; but at the promontory of Fairhead, the character of the ſtone is ſeen to alter, and it has lately been deſcribed as an imperfect kind of granite *. Hence we are led by regular approaches to perfect priſms of granite, accompanied by priſms of common whinſtone, and not leſs obviouſly than the different ranges on the Coaſt of Antrim betraying a common origin. The pillars of Les Rameaux, though they rather incline towards the dark colour and uniform hard ſubſtance; "yet, when broken, are unequal both in colour and texture, "and ſometimes interſperſed with irregular pieces and patches, "as it were, of an heterogeneous hard ſubſtance, which, by "its micæ and ſmall rhomboidal cryſtallizations, much re-"ſembles a ſort of granite I have frequently ſeen. The "maſs on which theſe columns ſtand is of the ſame mixed "character †." Other examples will occur afterwards; and for baſaltiform colonnades of granite it is only neceſſary to refer to Mr. STRANGE's deſcription of Monte Roſſo ‡. The general ſhape of the Euganean hills, as if ſuddenly raiſed by

* HAMILTON, 2d edition of his Letters, p. 37. ſays, it "reſembles an imperfect compact granite." I have ſpecimens from an hill near Mallwhyd, in the vicinity of Cader Idris, where the texture inſenſibly changes from an uniform whinſtone ground to grains of mica, feldſpath, ſhoerl, and, I think, ſometimes quartz. They are, I fancy, like thoſe denſe lavas of Etna, "Qui, vues à la loupe, laiſſent appercevoir des ébauches de cryſtallization de ſhörl, de quarz, ou de feld-ſpath. DOLOMIEU, *l. c.* p. 182.

† STRANGE, Phil. Tranſ. LXV. p. 13.

‡ Ibid.

the expanſive and effervescent force of heat * from the ſurrounding plain, the lava intermixed with granite, as if both had concreted together, the columns of an uniform texture in the adjacent parts of theſe hills, and the reſt of the phænomena, even then led the author to ſuſpect, "a ſtrong analogy be-"tween granites and many particular volcanic concretions."

From the mountain of Eſterelles, in the South of France, on the road from Frejus to Antibes, I have before me granite, gneiſs, and ſpecimens, in which feldſpath and grains of tranſparent quartz are diffuſed through a paſte of the ſame browniſh red colour and texture as the baſaltic columns at Dunbar in Scotland.

Nothing is indeed more common, or more variouſly modified, than foſſils of this intermediate character †. We frequently find a ground of jaſper, and no doubt alſo of different varieties of whinſtone, as will hereafter appear, with feldſpath and ſhoerl at the ſame time imbedded in them ‡; and again with grains of feldſpath and quartz in ſuch a manner as to leave it extremely doubtful, whether the rock ought to be named granite or porphyry §. The varieties of ſuch rocks will conduct us, by eaſy ſteps, from uniform baſaltes through the porphyries to granite. A chemical examination of the baſis of a number of theſe porphyries would be very intereſting; yet I would not reſt the theory of their formation altogether on the reſult of analyſis.

* The ſight of them impreſſed FERBER alſo with the ſame idea.

† Il n'y a point de naturaliſtes qui ne connoiſſent le genre de roches, placé entre le granit, les gneiſs, et les porphyres, qui tiennent un peu des trois eſpeces. Iſles Ponces, p. 90.

‡ HAIDINGER, Tract quoted below, p. 47.

§ CHARPENTIER (Mineralogical Geography of Saxony, 4to, 1778. in German), p. 50. and elſewhere, finds himſelf in this ſtate of uncertainty.

The

The ſame ſtratum is perpetually varying in its mixture; and we ſhould not too rigouroufly adhere to the proportion of ingredients diſcovered by the chemiſt in the hundred grains upon which his experiments may chance to be made. The ſenſible qualities, the ſtile of fiſſure, the accompanying foſſils, and the form of whole rocks, when ſurveyed by an experienced eye, are as good criterions of baſaltes as a certain proportion of iron, and the black glaſs which it yields on fuſion. Should the matter of any given rock contain too little iron to be fuſible by the blow-pipe, and yet have other ſtriking features of whinſtone, would this be a ſufficient reaſon to conclude, that its formation has been different? Chemiſtry, if thus ſtrictly followed, would perplex mineralogy, inſtead of reducing it to order. Characters of minerals, purely chemical, would ſeparate thoſe whoſe natural hiſtory is alike, and bring together ſuch as differ widely in their formation.

The late Mr. FERBER's letters from Italy furniſh ſo many facts, conſpiring in one way or another to ſhew the affinity between baſaltes as well as other products of ſubterraneous fire and granite, that whoever reads them with this view will, I am perſuaded, find himſelf more intereſted and inſtructed. The following are among the moſt ſtriking of theſe facts.

"4th ſpecies of baſaltes. Oriental baſaltes through which "the conſtituent parts of granite are equably diffuſed. Sepa- "rate particles of red feldſpath, quartz, and mica, are "diſperſed through the ſubſtance of this ſpecies: they ſeem to "have been diſtributed through an aqueous ſolution, and to "prove, that this ſpecies had rather an aqueous than a fiery "origin." I ſee neither proof nor preſumption in favour of this ſuppoſition; but in a ſeries of ſpecimens, collected with a view

view to shew the transition from black basaltes to granite, this species and the granite from Esterelles would form two contiguous links.

"5th Oriental basaltes, with stripes of granite. The "common black basaltes, fasciated with large stripes of red "granite, blended and joined to the basaltes without any "visible separation; not as the pebbles in a breccia, or as "fissures healed up and filled with granite, but as if both the "basaltes and granite had been fluid together *."

Those specimens, which shew how copiously volcanos produce feldspath, shoerl, and mica, especially the two former (substances common both to basaltes and granite), tend greatly to establish the near relation between these two kinds of rock. I was surprized, at this day, to find an excellent observer seriously maintaining, that these earthy crystallizations are merely ejected, and not generated, by these fires †.

Attempts, I am aware, have been made to set up boundaries between the columnar granite of the Euganean hills, the granite of the volcanic provinces of France, the granitello of the Italians, and such granite as is found to constitute high and extensive ranges of mountains. As to a difference in the size of particles, and hardness of the stone, the first distinction is neither constant, nor by any means calculated to persuade us, that a cause, capable of producing the one, is inadequate to the production of the other. It may probably be explained from the quantity of matter, more or less perfect fusion, a different length of time in cooling; and in the latter character I suspect the observers to have been deceived by the

* See FERBER's Travels in Italy, pp. 231, 232. English translation.

† DOLOMIEU, Isles Ponces, and Laves de l'Etna, passim.

decay

decay of the rocks they inſpected *. At all events, lavas in abundance ſhew, that fire is capable of producing any required degree of compactneſs †.

I ſhall conclude this induction of particulars with an obſervation lately publiſhed by one of our moſt intelligent mineralogical travellers. "Among the ancient black ſtones, the "compound ſpecies are moſt frequent. They often conſiſt of "*a kind of granite*, in which the ſcaly black ſhoerl predominates "ſo much, that the whole maſs appears black. It is accompa-"nied by white feldſpath of ſo ſmall a grain, and ſo entangled "among the ſhoerl, as to be ſometimes ſcarcely diſtinguiſhable. "The feldſpath itſelf is ſometimes tranſparent, and by tranſ-"mitting the colour of the ſhoerl, in which it is imbedded "(empâté) appears black . . . . . Sometimes ſcales of black "mica occur. The conſtituent parts do not always obſerve "the ſame proportion; and when the quantity of feldſpath "increaſes, the appearance of a real grey or red granite is pro-"duced. Hence we have veins and ſpots of grey granite in "almoſt all the dark-coloured rocks that paſs under the "denomination of baſaltes. Theſe veins have very much "embarraſſed thoſe naturaliſts who maintain that all baſaltes "has been produced by fire." This circumſtance, however, according to my view of the ſubject, is far from embarraſſing: I conſider it as a ſtrong proof of my opinion, ſince it ſeems to involve this conſequence, that, if baſaltes proceed from fuſion, granite alſo muſt. Specimens, ſuch as thoſe here deſcribed, I ſhould place near granular baſaltes, like that of Cape Fairhead. "In blocks of ancient baſaltes," proceeds M. Dolo-

* "Though partial ſpots of this granite are often friable, eſpecially about the ſurface, yet in general it is very hard." Strange, ubi ſupra, p. 9.

† See Dolomieu, Iſles Ponces, p. 139.

MIEU,

MIEU, "I have obſerved the tranſition from ſhoerl in a "maſs nearly homogeneous (I ſay, *nearly homogeneous*, be- "cauſe I know of no ſtones, belonging, as theſe do, to the "primitive mountains, without indications of a ſeparation of "ſeveral ſubſtances which were incorporated together in a "paſte, or rather which are generated in that paſte) to black "and white granites, with large grains, and compoſed of "nearly equal quantities of white feldſpath and black ſhoerl. "The tranſition depends altogether on an increaſed proportion "of feldſpath and on the enlargement of its grains; a "phænomenon which leaves no room to doubt, that all theſe "ſtones belong to the ſame ſyſtem of mountains *."

By obſervations like theſe, which the ſpecimens I either poſſeſs, or have examined, corroborate and compleat, I am perſuaded, that when once it becomes an object of attention, perſons who have an opportunity of exploring countries where baſaltes and granite abound, will eaſily find a ſucceſſion of ſpecimens beginning at the former and terminating at the latter. Nor is it perhaps difficult to aſſign highly probable reaſons, why a mixture of different earths with more or leſs of metallic matter, in returning from a ſtate of fuſion to a ſolid conſiſtence, may aſſume ſometimes the homogeneous *baſaltic*, and ſometimes the heterogeneous *granitic* internal ſtructure. No fact is more familiar than that it depends altogether on the management of the fire, and the time of cooling, whether a maſs ſhall have the uniform vitreous fracture, or an earthy broken grain, ariſing from a confuſed cryſtallization. The art of making REAUMUR's porcelain conſiſts en-

* Journal de Phyſique, September 1790, p. 196. I have ſeveral ſpecimens from the whin rocks about Edinburgh perfectly anſwering to this deſcription, and ſo like ſome Veſuvian lavas that it is impoſſible to diſtinguiſh them.

tirely in allowing the black glaſs time to cryſtallize by a ſlow refrigeration; and the very ſame maſs, according as the heat is conducted, may, without any alteration of its chemical conſtitution, be ſucceſſively exhibited any number of times as glaſs, or as a ſtony matter with a broken grain. In the ſlag of the iron furnaces, the ſame piece generally exhibits both theſe appearances; the upper ſurface cools faſt, and is glaſs; what lies deeper, loſes its heat more gradually, and is allowed time to take on the cryſtalline arrangement peculiar to its nature, in as far as a number of cryſtals, ſtarting from various points at once, and crowding each other, will admit of it. Here indeed the cryſtals are uniform, and not of a different form and compoſition, as in granite; ſo that this analogy applies cloſely only to baſaltes; and it perfectly explains why this body in congealing has aſſumed an earthy and not a vitreous grain. But it is eaſy to conceive how, under certain variations of heat and mixture, a melted maſs may coagulate into quartz, feldſpath and ſhoerl, or mica*. The moſt permanent difference between baſaltes and granite, as to mixture, conſiſts in the quantity of iron; for the earths in the innumerable varieties of each vary indefinitely in their proportions; and as to heat, that the latter having been perhaps in general raiſed from a greater depth, and conſiſting of more

* "Il eſt certain que dans les porphyres les criſtaux de feld-ſpath n'exiſtoient pas avant l'époque de la précipitation de leur baſe." He ſuppoſes them to be of aqueous origin; but the fact is much to my preſent purpoſe. "On y ſuit les progrès ſucceſſifs de leur formation: on voit que peu à peu les ſubſtances qui leur ſont propres ſe rapprochent, s'epurent et prennent les formes qui conviennent à leurs molécules; ils étoient comme en diſſolution dans leurs matrices et ils ont d'autant plus de facilité à ſe joindre que la fluidité a été plus parfaite et que le deſſechement a été plus long." Iſles Ponces, 247, 248.

huge maſſes, muſt have cooled more ſlowly, and perhaps they have undergone different degrees of fuſion. Beſides toadſtone, baſaltes incloſing feldſpath, zeolite, &c., various lavas clearly demonſtrate that heterogeneous earthy cryſtals do ſeparate from a fuſed paſte, once undoubtedly as uniform as a metallic calx and its reducing flux before the ſubſidence of the metallic particles. We ſhall, I imagine, be much deceived by a narrow analogy if, becauſe in our proceſſes for glaſs-making an homogeneous product is obtained from heterogeneous materials, we conclude, that an heterogeneous product may not, under other circumſtances, reſult from fuſion; and that fire keeps inſeparably blended whatever it has once reduced to an uniform liquid paſte.

It muſt alſo be carefully remembered, that this difficulty does not preſs the igneous more than the oppoſite hypotheſis. Since the conſtituent parts of granite are cryſtals, the whole maſs muſt once have exiſted in that ſtate of entire diſunion of its particles which is neceſſary to cryſtallization. Now, whether ſuch a ſolution have been effected by the repulſive power of fire, or the intervention of water, it is juſt as eaſy to conceive heterogeneous earthy cryſtals ſhooting from different points of an uniform liquid, according to the former ſuppoſition, as the latter.

In the natural hiſtory of granite and baſaltes, another ſtriking circumſtance occurs: *they lie ſo contiguous, and are ſo involved in one another, that we cannot but ſuppoſe both to have undergone the ſame operations of nature at the ſame time.* This is ſeen with the utmoſt frequency upon every poſſible ſcale, and under a vaſt variety of modifications. The facts already quoted afford inſtances in point. I have before me a ſpecimen from the park at Stockholm, conſiſting partly of trapp and partly of granite. The adjacent parts are as firmly united as the other

parts of the ſpecimen; and when a violent blow is ſtruck, the trapp and granite do not ſeparate, but the fracture takes ſome other direction. They ſeem in ſeveral places of the boundary to run into one another. The whole mountainous diſtrict ſurveyed by Mr. Leske * with ſuch ſcrupulous accuracy affords multiplied examples of the contiguity and connection between theſe different rocks. "From all theſe minute "deſcriptions," ſays the Author, "it appears, that the baſe "of the whole range conſiſts of granite. On the declivity of "the higheſt elevations, and on the ſolitary ſummits of the "external chain, *corneous porphyry* lies upon the granite, out "of which as well as the granite itſelf, and the ſandſtone at "its foot, baſaltes has been protruded by the force of ſubter-"raneous fire †." The manner of connection will appear from a few examples. The baſaltes of the Spizberg ‡ has a granulated ſtructure, and is imbedded in granite. The ſubſtance of the pillars of the Gikelſberg § is cloſe and granular: in ſome pieces "I found the conſtituent grains of granite *little* "*altered*." Of the columns of the Knorberg, "the ſubſtance "is cloſe, uneven, and conſiſts of diſtinct grains: . . . large "pieces of *imperfectly fuſed* granite are diffuſed through its "ſubſtance. In the Whinſtone of the Hochwald there are "found pieces conſiſting of a mixture of white feldſpath, "quartz, and black ſhoerl ‖." Again, in the Rauberg, the conſtituent parts of granite are ſo diffuſed through the baſaltes,

* Reiſe durch Sachſen.

† Ibid. p. 513. "It is alſo remarkable, that granite in general throughout Velay and Auvergne is frequently intermixed with the baſaltine and other common volcanic hills. I have obſerved the ſame in Italy." Strange, p. 14.

‡ Reiſe durch Sachſen, p. 328.

§ Ibid. p. 498. ‖ Ibid. p. 515.

that the Author imagines the rock to be an imperfectly fused granite *. I rather consider these as instances of imperfectly crystallized granite, where some unfavourable circumstance has prevented the constituent parts from receding completely from one another. Experiments shew, that almost all granites melt into a black glass †; and perhaps it is no abuse of analogy, nor inconsistent with what I have already remarked, to conclude, that granite, in the state of imperfect fusion, should present a glassy substance, involving the more infusible parts of which this stone consists.

The Scheibenberg, near Königsbruck, consists of a stone which Mr. LESKE knows not whether to call hornslate ‡, or corneous porphyry §. From the description it appears plainly to be a whinstone. The colour is dark grey; it breaks into columnar fragments; is hard, fine-grained, and *sonorous*; little veins of quartz cross it in all directions, and it frequently becomes porphyritic, as enclosing crystals of feldspath. The Author himself is afterwards aware ‖ of its affinity to basaltes, both in substance and from its assuming the columnar form. In this hill a mass of granite is found imbedded in the whinstone, and on all sides surrounded by it, and the mass of granite is in its turn in all directions intersected with veins and stripes of whinstone. Mr. LESKE is much struck by this mutual and intimate incorporation; but he makes no attempt to explain it. In some instances, he thinks an eruption has

* Reise durch Sachsen, p. 330.

† HAIDINGER's Eintheilung der Gebirgsarten. Wien, 1787, p. 11. and the Authors quoted there.

‡ Hornschiefer.

§ Ibid. p. 24—29. No chemical characters are given.

‖ Ibid. p. 513, 514

broke out through the granite; and in others, is at pains to ſhew, that theſe ſubſtances are not thoroughly blended, as in the laſt example, and in that deſcribed by FERBER.

It may be ſaid, and no doubt it ſometimes happens, that ſhivers of granite, broken off by the violence of exploſion, are licked up by melted matter as it moves along; thus, in volcanic breccias an older lava is incloſed in one more recent, and thus what is called primary is ſometimes encaſed in ſecondary granite. But ſuch an hypotheſis is too narrow to embrace all the phænomena. It does not explain the incipient coagulation of the uniform paſte into grains, and thoſe the different grains of granite; nor the diffuſion of the conſtituent parts of granite through the ſubſtance of baſaltes; nor the fifth ſpecies deſcribed by Mr. FERBER.

In the whinſtone rocks of England, which are far more numerous than is commonly ſuppoſed, I have frequently obſerved in the ſame hill, 1. homogeneous dark grey ſtone; 2. feldſpath incloſed in this as in a paſte; and, 3. the paſte diſappearing, and the whole becoming granular, and the grains heterogeneous. Beſides feldſpath, quartz is found in innumerable maſſes of varying magnitude * in many whinſtone rocks, and as proper baſaltes is but a confuſed maſs of cryſtals of ſhoerl, we have all the ingredients of granite; and why

* In the Wrekin, Cader Idris, &c. numerous and large veins filled with quartz occur; but theſe have not been *ſecreted* from the ſubſtance of the rocks in which they lie. They, perhaps, ſhew this moſt ſiliceous ingredient of granite to have been near and in fuſion at the ſame time. About Cader Idris I ſuſpect an incorporation of granite and baſaltes may be found. I have ſeen pieces of granite about that mountain which did not ſeem to have been far removed from the rock to which they belonged; but I had then no particular inducement to make an accurate examination.

may,

may we not expect to find them incorporated together, and in every ſtate of diffuſion and ſeparation?

Under this head I ſhall only remark further, that ſeveral late obſervations, from which it has been inferred, that certain extinct volcanos have been ſeated in the heart of granite *, ſeem to admit of a much more eaſy explanation, on the ſuppoſition that granite has cryſtallized from fuſion. 1. Volcanic fires reach to a much greater depth than any at which we have had an opportunity of making obſervations. The focus in different inſtances may be ſeated at a different diſtance from the ſurface; but none are probably leſs than ſeveral miles at leaſt deep. 2. The currents of granitic lava † in the Pontian iſles leave little room to doubt of the power of ſubterraneous fire to produce this ſubſtance. To ſuppoſe them to be rocks of granite fuſed, but otherwiſe unchanged, and that even fiſſile rocks may be made to flow without loſing their laminated ſtructure; is as bold an aſſumption as can eaſily be taken up. In the great igneous proceſſes of nature fire need not be imagined to act otherwiſe ‡

* DOLOMIEU, Iſles Ponces, p. 30, 31. and Iſles de Lipari, in various parts.

† DOLOMIEU, *l. c. paſſim*, and particularly p. 89—97. Inſtructive as the particular facts deſcribed by this excellent mineralogiſt always are, I muſt diſſent from him both on the mode of action of volcanic fires, and on the production of zeolite and other cryſtals, in glandular rocks, for the reaſons aſſigned here and below.

‡ "Le feu produit, dans les laves, une fluidité qui n'a aucun rapport avec la fluidité vitreuſe que nous opérons . . . . . Celui des volcans n'a point d'intenſité; il ne peut pas même vitrifier les ſubſtances les plus fuſibles . . . . il produit la fluidité par une eſpece de diſſolution, par une ſimple dilatation qui permet aux parties de gliſſer les unes ſur les autres (DOLOMIEU, Avant-propos, p. 8.). See alſo p. 84. Fire in a crucible produces fluidity no otherwiſe; and when there is this freedom of motion among the *particles*, how can we ſuppoſe the cryſtals of granite and the leaves of ſchiſtus to remain unmixed even in a current of lava?

than

than in our ſmall experiments; we actually ſee it producing glaſs and cellular ſpongy ſcoriæ: when the products are of a different character, we muſt have recourſe to acceſſory circumſtances, and not violate the plaineſt rules of philoſophiſing by attributing different effects to the ſame cauſe. The latent motive for ſuch an extraordinary hypotheſis may eaſily be divined; the obſerver took it for granted, that all granite is of aqueous formation; hence he was obliged to reaſon backwards from the unknown, that of the Alps for inſtance, to the known, inſtead of proceeding from the palpable effects of ſubterraneous fire by eaſy ſteps to a general theory of granite. When it is taken for granted, before examination, that granite cannot be formed by fire, there remains no reſource but to ſay, that granitic * lavas are granite rocks fuſed, but not altered in the arrangement of their conſtituent parts. 3. Though the heat of volcanos be ſometimes and in ſome places moderate; in others we have good reaſon to believe, that it exceeds any degree we can produce, except by means of factitious air; we are certain that it forms molten currents of petroſilex and flint exactly the ſame as our gun flints †.

If we admit this reaſoning, the appearance of granite in the boſom of volcanic deſolation may, if duly examined in all its circumſtances, afford ſtrong evidence of its production by fuſion; and it is reaſonable to conclude, that it was once covered to a conſiderable depth by erupted matters, which the courſe of

* "Les laves blanches de l'Ile Ponce paroiſſent appartenir plus particuliérement au granit et aux roches feuilletées granitiques. On reconnoît les ſubſtances qui conſtituent ordinairement ce genre de roche compoſée dans preſques toutes les matières blanches (volcaniques) de cette iſle; ſavoir, le quartz en grain, le mica noir écailleux et le feldſpath plus ou moins pur. Dolomieu, *l. c.* p. 89.

† Idem ibid. p. 107.

time,

time, and the injuries of the atmosphere, have removed; though I by no means deny that a volcano may force its way through pre-existing rocks of granite.

There is still another analogy between basaltes and granite, more important to the theory of the earth, and less liable to controversy than either of the preceding. *In their situation, with respect to other rocks, we may observe the same law.* The general rule of super-position, reckoning from below upwards, is, 1. granite; 2. schistus; 3. limestone. This rule has been found to hold good by so many mineralogical travellers that, though it may not be absolutely universal, it must be allowed to prevail very extensively. Now, in this island there are numerous instances where basaltes is substituted in the series instead of granite, and where it seems to alternate with granite as the substratum of other rocks. On the road from Dolgelly in Merionethshire, by Mallwhyd and Cann's Office, through Llanfair to Welchpool, schistus appears always incumbent on whinstone, except sometimes when the latter is interjected between the strata, or squeezed up through fissures. In Wales the country is so hilly, that the limestone, if it existed, has probably been washed away; but on the confines of England it comes in. The road from Welchpool to Shrewsbury passes over the side of the Long Mountain, which consists of schistus; on the left, or towards the east, rise some considerable basaltic hills. The strata of the Long Mountain point towards the summit of these hills, as if the narrow valley that intervenes had been cut by water upon the lifted edge of the schistus. At a small distance from the north and south sides of the basaltic hills calcareous strata are found. Beyond Shrewsbury, on the road to London, we have, instead of the continued ridges of Wales, a number of insulated, and generally

generally rugged, points, riſing over the face of Shropſhire and the adjacent counties. Were the plains covered with water a few yards in depth, theſe eminences would appear from diſtance to diſtance like ſo many ſtepping ſtones. They all, except the Malvern Hills, which, though compoſed of granite, I conſider as part of the ſame ſyſtem, conſiſt of whinſtone. Among theſe ſtepping ſtones I reckon the baſaltic hills near Welſhpool, the Wrekin, Lilleſhall Hill, and, at a greater diſtance towards the Eaſt, the riſing grounds near Newcaſtle in Staffordſhire, whence the whin rock, perhaps, communicates by the toadſtone of Derbyſhire, through the hills in the North of England with the whinſtone towards the South of Scotland. In a ſouth or ſouth-weſt direction from the Wrekin, a number of craggy eminences ariſe. They are baſaltes, and form a ſtriking contraſt with the ſmooth, rounded, and lumpiſh ſwells of ſchiſtus in their neighbourhood. From the whin rocks near Stretton we may paſs by the Brown and Titterſtone Clee Hills (on the latter of which are regular priſmatic columns) to the Malvern Hills. About theſe hills lie ſtrata of ſchiſtus and limeſtone, as is ſeen on the road from Much Wenlock to Stretton. To the ſouth-eaſt an extenſive field of whinſtone, with occaſional elevations, is ſpread over the confines of Worceſterſhire, Warwickſhire, and Staffordſhire. Here we have the Rowley ragſtone. Whether the baſaltes proceeds ſouthward by ſuch interruptions till it join the Elvin or whinſtone, and granite of Devonſhire and Cornwall, where I imagine they may be found incorporated, I wiſh for an opportunity to examine. In the plain part of this whole diſtrict, the whin rock appears frequently at the ſurface, or a little below the ſtrata, ſo that the hills have probably a ſubterraneous communication with one another, and there needed

but a little more lifting force to form continued ranges of mountains. The road from Welſhpool to Birmingham, above threeſcore miles, is repaired in a great meaſure with whinſtone. A colonnade of baſaltes has been lately expoſed in digging the Shropſhire canal; and in the mining country around, levels have been driven in the black rock, as it is ſometimes called. As I have ſeen whinſtone and ſlate in various other parts of North and South Wales, the whole weſtern ſide of our iſland has probably been raiſed by the baſaltes on which the ſuperficial ſtrata now reſt, though from particular circumſtances the fuſed maſs has now and then cryſtallized into granite; and as it has been conjectured, that the baſaltes of Ireland once joined that of the Scotch iſles and the main land itſelf, ſo perhaps the baſaltes of North Wales joined the Iriſh coaſt till the ſea worked its way or broke in, and deſtroyed the continuation.

As limeſtone is ſometimes ſaid to reſt immediately on granite *, ſo at the foot of the Wrekin, and at Lilleſhall Hill, no ſlate is interpoſed between the limeſtone and baſaltes; ſo that the analogy extends even to the exceptions.

But another ſeries has been obſerved, which ſeems to connect granite by a cloſer tie with the operations of ſubterraneous fire. In Italy lava ſtands to ſlate and limeſtone in the ſame relation as granite and whinſtone in other countries †. Whole ridges of mountains in the Venetian territory conſiſt of ſolid lava, ſometimes almoſt bare, ſometimes retaining the ſuperincumbent ſtrata, with ſeveral local variations; all of which are reducible to a greater or leſs degree of lifting force. Theſe

* BORN's Letter, p. 207.

† FERBER, *l. c.* pp. 42. 51.; and eſpecially STRANGE, *l. c.* p. 24—32.

chains

chains have a totally different form from the common conical ſhape of volcanos or heaps of looſe ejected matters. They ſeem to afford a clear inſtance of the manner in which long continuations of mountains have been elevated; for it is not eaſy to admit the ſuppoſition of the obſerver, who has ſo accurately deſcribed them, that the limeſtone has been converted into lava; and that the ridges exiſted, ſuch as they appear at this day, before this change was produced by ſubterraneous fire. Chemical and mechanical conſiderations are unfavourable to this hypotheſis; and "ſince moſt of theſe branches, whether "marine, volcanic, or mixed, preſerve nearly the ſame exter-"nal characters, directions, and parallelism;" it appears highly probable, that they have not pre-exiſted as hills in another ſtate, but owe their elevation to the expanſive force of fire; and that the ſame lava which appears in ſo many places lies alſo under all the limeſtone hills, of which indeed there are evident indications.

Several modern travellers have deſcribed the ſtrata of granite mountains; but neither in their deſcriptions nor drawings have I been able to find ſatisfactory evidence of this arrangement; nor have I obſerved it in nature. A liquid maſs ſwelled by heat muſt crack in cooling. Granite ſeems to me to have cracked moſt frequently like the *baſalte en tables*; and theſe flat maſſes have been taken for ſtrata. A ſtratum, conſiſting of proper materials to form whinſtone or granite, may have been expoſed to the neceſſary degree of heat, and poſſibly have undergone this change without much relative local derangement. Should ſuch a ſtratum be diſcovered, it would afford no proof of the ſtratification of the great mountains of granite or ſhapeleſs whinſtone, which, in conſequence of its nu-

merous fiſſures in all directions, ſometimes aſſumes enough of this appearance to impoſe on an unwary eye.

One conſequence of theſe obſervations is too important to be omitted. They lead us to reject the common diviſion of mountains into primary and ſecondary. The chains of granite, ſchiſtus, and limeſtone, muſt be all coeval; for if the central chain of the Alps burſt as a body expanded by heat from the bowels of the earth, it reared the bordering chains at the ſame effort. But it muſt be recollected, that the mountains no longer wear their original form, vallies having been cut between and through them, and various other effects of dilapidation having taken place. It is by no means difficult to underſtand why no exuviæ of organized bodies are found in theſe imaginary primitive mountains. Riſing from a great depth, they threw aſide the ſuperficial accumulations of the ancient ocean. What was deepeſt is therefore now moſt central; and what lay on the ſurface now ſkirts the high interior chains. Hence the ſtrata reſt indifferently on granite, baſaltes, or lava; all which ſubſtances derive from their ſituation an equal claim to be regarded as primordial materials. It is a little ſurprizing, that this inveterate error, which has effectually barred the way to all great diſcoveries in geology till of late *, ſhould have prevailed ſo long: for, 1. it is well known, that granite is ſometimes found encloſing pieces of ſchiſtus †; nor are long ſtretches of ſlate uncommon in mountains of granite ‡. Now, how can a ſecondary be ſo enveloped in a primitive rock? and how eaſy is this to be underſtood, if we ſuppoſe granite as a fuſed maſs raiſing, rending,

* Till Dr. HUTTON's Theory of the Earth, Edinb. Tranſ. Vol. I. appeared.

† BORN's Letters, p. 207, 208.

‡ HAIDINGER, *l. c.* p. 18.

and ſhivering the incumbent ſtrata, while its heat hardened them into laminated ſtone. 2. Suppoſing granite mountains previouſly exiſting in the ancient ocean, the inclination of the incumbent ſtrata, and their diſarrangement is ſuch that they could never have been depoſited as they appear at preſent; they would have been much more horizontal in their direction. It ſeems impoſſible to attribute the diſorderly deviation, which is ſo general in the mountains of ſlate, &c. from that poſition which all ſediments from water aſſume, to any thing but a force lifting from below, and ſometimes burſting through.

It is moreover certain, that all theſe lifting maſſes, from granite to acknowledged lava *, are found ſqueezed up through fiſſures formed in the ſtrata by their own expanſion. This, and not the infiltration of water, as M. DE SAUSSURE would perſuade us †, appears to be the true origin of ſuch veins of granite.

* FERBER, *l. c.* p. 51.

† Voyages dans les Alpes, 4to, I. 532—536. The whole paſſage well deſerves the notice of thoſe who are intereſted in theſe enquiries. Vertical ſtrata, in one inſtance, lying againſt the foot of a granite mountain, are divided by oblique fiſſures, full of granite. This naturaliſt ſuppoſes them to have filled up gradually by the rain water diſſolving particles of granite, carrying them down and depoſiting them in the form of granite again; two operations which one may ſafely deny to rain water the power of performing. Other inſuperable objections to this theory ſtart up at every ſtep in the deſcription. If water can diſſolve any, it is ſurely but a very ſmall part of all the ingredients of granite. Now, ſuppoſe a fiſſure full of ſuch a ſolution; the water is, I ſuppoſe, to evaporate, and the cryſtallization to take place; the cryſtals muſt be ſmall, on account of the ſmall quantity of matter to form them, and a ſucceeding ſolution can only yield another crop of ſmall cryſtals, it will not enlarge thoſe already formed; but we are told, that the cryſtals of granite in the crevices in queſtion are remarkably large. This has always appeared to me a chemical demonſtration of the falſehood of a very common ſuppoſition, that the cryſtals often occurring in the cavities of the

the amygdaloides rocks have been formed by the infiltration of water. The cryſtals are frequently of ſo large a ſize that you can by no means ſuppoſe the quantity of water, at any one time exiſting in the cavity, could have held the ſolid matter, of which they conſiſt, in ſolution. Now, I think, it is contrary to all our experience in chemiſtry, to ſuppoſe cryſtals built up by ſucceſſive operations. If upon cryſtals of nitre I pour a ſolution of the ſame ſalt, the former cryſtals will not be enlarged and amended, but a new ſet will be formed; ſo ſucceſſive quantities of water, paſſing through theſe cavities, ought to form ſucceſſive ſets of very ſmall cryſtals. Neither can I imagine, what cauſes can produce within theſe cavities a depoſition of the matter once diſſolved by the water. It is not cooled; it does not evaporate; it loſes no fixed air; it comes in contact with no new matter, whoſe attraction may overpower the attraction of the water.

The diviſions, "rather marked out than formed, which croſs each other irregularly (p. 533.), and indicate an incipient retraction," are much more conſiſtent with a ſimultaneous congelation than a gradual appoſition; and the granite is the ſame as that of the contiguous hill in the colour and appearance of its conſtituent parts. This coincidence is a ſtriking fact.

In truth, the philoſophers who attribute the formation of granite to water ſeem not to have advanced a ſtep in their proof beyond the equivocal circumſtance of its being a cryſtallized maſs.

IV. *On Nebulous Stars, properly ſo called.*

By William Herſchel, *LL.D. F. R. S.*

Read February 10, 1791.

IN one of my late examinations of a ſpace in the heavens, which I had not reviewed before, I diſcovered *a ſtar of about the 8th magnitude, ſurrounded with a faintly luminous atmoſphere, of a conſiderable extent.* The phænomenon was ſo ſtriking that I could not help reflecting upon the circumſtances that attended it, which appeared to me to be of a very inſtructive nature, and ſuch as may lead to inferences which will throw a conſiderable light on ſome points relating to the conſtruction of the heavens.

Cloudy or nebulous ſtars have been mentioned by ſeveral aſtronomers; but this name ought not to be applied to the objects which they have pointed out as ſuch; for, on examination, they proved to be either mere cluſters of ſtars, plainly to be diſtinguiſhed with my large inſtruments, or ſuch nebulous appearances as might be reaſonably ſuppoſed to be occaſioned by a multitude of ſtars at a vaſt diſtance. The milky way itſelf, as I have ſhewn in ſome former Papers, conſiſts intirely of ſtars, and by imperceptible degrees I have been led on from the moſt evident congeries of ſtars to other groups in which the lucid points were ſmaller, but ſtill very plainly to be ſeen; and from them to ſuch wherein they could but barely be ſuſpected, till I arrived at laſt to ſpots in which no trace of

a ſtar was to be diſcerned. But then the gradations to theſe latter were by ſuch well-connected ſteps as left no room for doubt but that all theſe phænomena were equally occaſioned by ſtars, variouſly diſperſed in the immenſe expanſe of the univerſe.

When I purſued theſe reſearches, I was in the ſituation of a natural philoſopher who follows the various ſpecies of animals and inſects from the height of their perfection down to the loweſt ebb of life; when, arriving at the vegetable kingdom, he can ſcarcely point out to us the preciſe boundary where the animal ceaſes and the plant begins; and may even go ſo far as to ſuſpect them not to be eſſentially different. But recollecting himſelf, he compares, for inſtance, one of the human ſpecies to a tree, and all doubt upon the ſubject vaniſhes before him. In the ſame manner we paſs through gentle ſteps from a coarſe cluſter of ſtars, ſuch as the Pleiades, the Præſepe, the milky way, the cluſter in the Crab, the nebula in Hercules, that near the preceding hip of Bootes (*a*), the 17th, 38th, 41ſt of the 7th claſs of my Catalogues (*b*), the 10th, 20th, 35th of the 6th claſs (*c*), the 33d, 48th, 213th of the 1ſt (*d*), the 12th,

(*a*) RA. 13 h. 27′ 40″. PD. 60° 2′. The places of all the objects mentioned in this Paper are not brought to the preſent time, but given as they were calculated from the beſt obſervations I have made of them; the change in their ſituation ariſing from the lapſe of a few years is too trifling to be any hindrance to our finding them very eaſily.

				h.	′	″		°	′
(*b*)	VII.	17.	RA.	7	9	45.	PD.	114	34.
		38.		6	53	16.		88	37.
		41.		22	20	20.		38	47.
(*c*)	VI.	10.		16	14	22.		115	32.
		20.		0	42	4.		117	46.
		35.		0	19	44.		29	41.
(*d*)	I.	33.		11	57	26.		78	25.
		48.		17	10	46.		107	36.
		213.		12	17	59.		44	45.

150th,

150th, 756th of the 2d (*a*), and the 18th, 140th, 725th of the 3d (*b*), without any hesitation, till we find ourselves brought to an object such as the nebula in Orion, where we are still inclined to remain in the once adopted idea, of stars exceedingly remote, and inconceivably crowded, as being the occasion of that remarkable appearance. It seems, therefore, to require a more dissimilar object to set us right again. A glance like that of the naturalist, who casts his eye from the perfect animal to the perfect vegetable, is wanting to remove the veil from the mind of the astronomer. The object I have mentioned above, is the phænomenon that was wanting for this purpose. View, for instance, the 19th cluster of my 6th class (*c*), and afterwards cast your eye on this cloudy star (*d*), and the result will be no less decisive than that of the naturalist we have alluded to. Our judgement, I may venture to say, will be, that *the nebulosity about the star is not of a starry nature.*

But, that we may not be too precipitate in these new decisions, let us enter more at large into the various grounds which induced us formerly to surmise, that every visible object, in the extended and distant heavens, was of the starry kind, and collate them with those which now offer themselves for the contrary opinion.

			h.	′	″		°	″
(*a*)	II.	12. RA.	12	32	37.	PD.	72	23.
		150.	14	19	52.		81	43.
		756.	14	51	42.		35	22.
(*b*)	III.	18.	12	41	7.		84	51.
		140.	1	5	8.		92	0.
		725.	12	6	57.		43	14.
(*c*)	VI.	19.	15	5	2.		110	14.
(*d*)			3	56	48.		59	50.

It has been obſerved, on a former occaſion, that all the ſmaller parts of other great ſyſtems, ſuch as the planets, their rings and ſatellites, the comets, and ſuch other bodies of the like nature as may belong to them, can never be perceived by us, on account of the faintneſs of light reflected from ſmall, opaque objects; in my preſent remarks, therefore, all theſe are to be intirely ſet aſide.

A well connected ſeries of objects, ſuch as we have mentioned above, has led us to infer, that all nebulæ conſiſt of ſtars. This being admitted, we were authorized to extend our analogical way of reaſoning a little farther. Many of the nebulæ had no other appearance than that whitiſh cloudineſs, on the blue ground upon which they ſeemed to be projected; and why the ſame cauſe ſhould not be aſſigned to explain the moſt extenſive nebuloſities, as well as thoſe that amounted only to a few minutes of a degree in ſize, did not appear. It could not be inconſiſtent to call up a teleſcopic milky way, at an immenſe diſtance, to account for ſuch phænomena; and if any part of the nebuloſity ſeemed detached from the reſt, or contained a viſible ſtar or two, the probability of ſeeing a few near ſtars, apparently ſcattered over the far diſtant regions of myriads of ſidereal collections, rendered nebulous by their diſtance, would alſo clear up theſe ſingularities.

In order to be more eaſily underſtood in my remarks on the comparative diſpoſition of the heavenly bodies, I ſhall mention ſome of the particulars which introduced the ideas of *connection* and *diſjunction:* for theſe, being properly founded upon an examination of objects that may be reviewed at any time, will be of conſiderable importance to the validity of what we may advance with regard to my lately diſcovered nebulous ſtars.

On June the 27th, 1786, I saw a beautiful cluster of very small stars of various sizes, about 15′ in diameter, and very rich of stars (*a*). On viewing this object, it is impossible to withhold our assent to the idea which occurs, that these stars are connected so far one with another as to be gathered together, within a certain space, of little extent, when compared to the vast expanse of the heavens. As this phænomenon has been repeatedly seen in a thousand cases, I may justly lay great stress on the idea of such stars being connected.

In the year 1779, the 9th of September, I discovered a very small star near ε Bootis (*b*). The question here occurring, whether it had any connection with ε or not, was determined in the negative; for, considering the number of stars scattered in a variety of places, it is very far from being uncommon, that a star at a great distance should happen to be nearly in a line drawn from the sun through ε, and thus constitute the observed double star.

The 7th of September, 1782, when I first saw the planetary nebula near ν Aquarii (*c*), I pronounced it to be a system whose parts were connected together. Without entering into any kind of calculation, it is evident, that a certain equal degree of light within a very small space, joined to the particular shape this object presents to us, which is nearly round, and even in its deviation consistent with regularity, being a little elliptical, ought naturally to give us the idea of a conjunction in the things that produce it. And a considerable addition to this argument may be derived from a repetition of the same phænomenon, in nine or ten more of a similar construction.

(*a*) RA. 18 h. 20′ 2″. PD. 107° 3′.
(*b*) Phil. Trans. Vol. LXXII. p. 115. Catalogue of Double Stars, I. 1.
(*c*) RA. 20 h. 52′ 36″. PD. 102° 12′.

When I examined the cluſter of ſtars, following the head of the great dog (*a*), I found on the 19th of March, 1786, that there was within this cluſter a round, reſolvable nebula, of about two minutes in diameter, and nearly of an equal degree of light throughout (*b*). Here, conſidering that the cluſter was free from nebuloſity in other parts, and that many ſuch cluſters, as well as many ſuch nebulæ, exiſt in divers parts of the heavens, it appeared to me very probable, that the nebula was unconnected with the cluſter; and that a ſimilar reaſon would as eaſily account for this appearance as it had reſolved the phænomenon of the double ſtar near ε Bootis; that is, a caſual ſituation of our ſun and the two other objects nearly in a line. And though it may be rather more remarkable, that this ſhould happen with two compound ſyſtems, which are not by far ſo numerous as ſingle ſtars, we have, to make up for this ſingularity, a much larger ſpace in which it may take place, the cluſter being of a very conſiderable extent.

On the 15th of February, 1786, I diſcovered that one of my planetary nebulæ (*c*), had a ſpot in the center, which was more luminous than the reſt, and with long attention, a very bright, round, well defined center became viſible. I remained not a ſingle moment in doubt, but that the bright center was connected with the reſt of the apparent diſk.

In the year 1785, the 6th of October, I found a very bright, round nebula, of about 1½ minute in diameter (*d*). It has a large, bright nucleus in the middle, which is undoubtedly

		h.	′	″		°	′
(*a*)	RA.	7	32	1.	PD.	104	18.
(*b*)		7	32	5.		104	15.
(*c*)		17	58	25.		23	22.
(*d*)		3	30	35.		109	15.

connected

connected with the luminous parts about it. And though we muſt confeſs, that if this phænomenon, and many more of the ſame nature, recorded in my catalogues of nebulæ, conſiſt of cluſtering ſtars, we find ourſelves involved in ſome difficulty to account for the extraordinary condenſation of them about the center; yet the idea of a connection between the outward parts and theſe very condenſed ones within is by no means leſſened on that account.

There is a teleſcopic milky way, which I have traced out in the heavens in many ſweeps made from the year 1783 to 1789 (*a*). It takes up a ſpace of more than 60 ſquare degrees of the heavens, and there are thouſands of ſtars ſcattered over it: among others, four that form a trapezium, and are ſituated in the well known nebula of Orion, which is included in the above extent. All theſe ſtars, as well as the four I have mentioned, I take to be intirely unconnected with the nebuloſity which involves them in appearance. Among them is alſo *d* Orionis, a cloudy ſtar, improperly ſo called by former aſtronomers; but it does not ſeem to be connected with the milkineſs any more than the reſt.

I come now to ſome other phænomena, that, from their ſingularity, merit undoubtedly a very full diſcuſſion. Among the reaſons which induced us to embrace the opinion, that all very faint milky nebuloſity ought to be aſcribed to an aſſemblage of ſtars is, that we could not eaſily aſſign any other cauſe of ſufficient importance for ſuch luminous appearances, to reach us at the immenſe diſtance we muſt ſuppoſe ourſelves to be from them. But if an argument of conſiderable force ſhould now be brought forward, to ſhew the exiſtence of a luminous matter, in a ſtate of modification very different from the con-

(*a*) RA. from 5 h. 15′ 8″ to 5 h. 39′ 1′. PD. from 87° 46′ to 98° 10′.

ſtruction of a ſun or ſtar, all objections, drawn from our incapacity of accounting for new phænomena upon old principles, will loſe their validity.

Hitherto I have been ſhewing, by various inſtances in objects whoſe places are given, in what manner we may form the ideas of connection and its contrary by an attentive inſpection of them only: I will now relate a ſeries of obſervations, with remarks upon them as they are delivered, from which I ſhall afterwards draw a few ſimple concluſions, that ſeem to be of conſiderable importance.

To diſtinguiſh the obſervations from the remarks, the former are given in italics, and the date annexed is that on which the objects were diſcovered; but the deſcriptions are extracted from all the obſervations that have been made upon them.

October 16, 1784. *A ſtar of about the 9th magnitude, ſurrounded by a milky nebuloſity, or chevelure, of about 3 minutes in diameter. The nebuloſity is very faint, and a little extended or elliptical, the extent being not far from the meridian, or a little from north preceding to ſouth following. The chevelure involves a ſmall ſtar, which is about 1¼ minute north of the cloudy ſtar; other ſtars of equal magnitude are perfectly free from this appearance* (*a*).

My preſent judgement concerning this remarkable object is, that the nebuloſity belongs to the ſtar which is ſituated in its center. The ſmall one, on the contrary, which is mentioned as involved, being one of many that are profuſely ſcattered over this rich neighbourhood, I ſuppoſe to be quite unconnected with this phænomenon. A circle of three minutes in diameter is ſufficiently large to admit another ſmall ſtar, without any bias to the judgement I form concerning the one in queſtion.

(*a*) RA. 5 h. 57′ 4″. PD. 96° 22′.

It

It must appear singular, that such an object should not have immediately suggested all the remarks contained in this Paper; but about things that appear new we ought not to form opinions too hastily, and my observations on the construction of the heavens were then but entered upon. In this case, therefore, it was the safest way to lay down a rule not to reason upon the phænomena that might offer themselves, till I should be in possession of a sufficient stock of materials to guide my researches.

October 16, 1784. *A small star of about the 11th or 12th magnitude, very faintly affected with milky nebulosity; other stars of the same magnitude are perfectly free from this appearance.* Another observation mentions 5 *or* 6 *small stars within the space of* 3 *or* 4′, *all very faintly affected in the same manner, and the nebulosity suspected to be a little stronger about each star.* But a third observation rather opposes this increase of the faintly luminous appearance (*a*).

Here the connection between the stars and the nebulosity is not so evident as to amount to conviction; for which reason we shall pass on to the next.

January the 6th, 1785. *A bright star with a considerable milky chevelure; a little extended,* 4 *or* 5′ *in length, and near* 4′ *broad; it loses itself insensibly. Other stars of equal magnitude are perfectly free from this chevelure* (*b*).

The connection between the star and the chevelure cannot be doubted, from the insensible gradation of its luminous appearance, decreasing as it receded from the center.

(*a*) RA. 6 h. 0 ′ 33 ″. PD. 96 ° 13 ′.
(*b*) 5 30 53. 92 21.

January

January 31, 1785. *A pretty conſiderable ſtar, with a very faint, and very ſmall, irregular, milky chevelure; other ſtars of the ſame ſize are perfectly free from ſuch appearance* (*a*).

I can have no doubt of the connection between the ſtar and its chevelure.

October 5, 1785. *A ſtar with a ſtrong bur all around.* A ſecond obſervation calls it *a very bright nucleus, with a milky nebuloſity, of no great extent.* A third ſuſpects *the milkineſs to belong to more of the ſame, which is diffuſed over the whole ſweep in that place*; but a fourth ſays, that *the milky nebuloſity is much ſtronger than what the nebulous ground, on which the ſtar is placed, intitles it to* (*b*).

The connection, therefore, between the nebuloſity and the ſtar is evident.

January 1, 1786. *A ſtar ſurrounded with milky chevelure; the ſtar is not central.* A ſecond obſervation calls it *affected with a very faint, and extenſive, milky chevelure.* A third only mentions *a ſtar affected with milky chevelure* (*c*).

As by the word chevelure I always denoted ſomething relating to a center, the connection cannot be doubted.

February 24, 1786. *A conſiderable ſtar, very faintly affected with milky chevelure.* A ſecond obſervation, much the ſame (*d*).

November 28, 1786. *A ſtar involved in milky chevelure* (*e*).

	h.	′	″		°	′
(*a*) RA.	6	54	27.	PD.	100	53.
(*b*)	5	25	57.		96	52.
(*c*)	5	35	56.		89	50.
(*d*)	5	59	4.		96	19.
(*e*)	5	57	4.		96	15.

January

January 17, 1787. *A ſtar with a pretty ſtrong milky nebuloſity, equally diſperſed all around; the ſtar is of about the 9th magnitude.* A memorandum to the obſervation ſays, that, having but juſt begun, *I ſuſpected the glaſs to be covered with damp, or the eye out of order; but yet a ſtar of the 10th or 11th magnitude, juſt north of it, was free from the ſame appearance.* A ſecond obſervation calls it *one of the moſt remarkable phænomena I ever have ſeen, and like my northern planetary nebula in its growing ſtate* (*a*).

The connection between the ſtar and the milky nebuloſity is without all doubt.

November 3, 1787. *A bright ſtar with faint nebuloſity.* A ſecond obſervation mentions *the ſtar to be of the 9th magnitude, and the faint nebuloſity of very little extent* (*b*).

June 11, 1787. *Suſpected, ſtellar.* By a ſecond obſervation it is verified, and called *a very ſmall ſtar involved in extremely faint nebuloſity* (*c*).

November 25, 1788. *A ſtar of about the 9th magnitude, ſurrounded with very faint milky nebuloſity; other ſtars of the ſame ſize are perfectly free from that appearance. Leſs than 1′ in diameter. The ſtar is either not round or double* (*d*).

March 23, 1789. *A bright, conſiderably well defined nucleus, with a very faint, ſmall, round chevelure* (*e*).

The connection admits of no doubt; but the object is not perhaps of the ſame nature with thoſe which I call cloudy ſtars.

		h.	′	″		°	′	″
(*a*)	RA.	7	16	28.	PD.	68	39	
(*b*)		23	11	26.		30	0	
(*c*)		17	1	51.		47	26	
(*d*)		0	1	57.		18	41	
(*e*)		11	12	25.		50	17	

April 14, 1789. *A considerable, bright, round nebula; having a large place in the middle of nearly an equal brightness, but less bright towards the margin* (*a*).

This seems rather to approach to the planetary sort.

March 5, 1790. *A pretty considerable star of the 9th or 10th magnitude, visibly affected with very faint nebulosity of little extent, all around. A power of* 300 *shewed the nebulosity of greater extent* (*b*).

The connection is not to be doubted.

March 19, 1790. *A very bright nucleus, with a small, very faint chevelure, exactly round. In a low situation, where the chevelure could hardly be seen, this object would put on the appearance of an ill-defined, planetary nebula, of* 6, 8, *or* 10″ *diameter* (*c*).

November 13, 1790. *A most singular phænomenon! A star of about the 8th magnitude, with a faint luminous atmosphere, of a circular form, and of about* 3′ *in diameter. The star is perfectly in the center, and the atmosphere is so diluted, faint, and equal throughout, that there can be no surmise of its consisting of stars; nor can there be a doubt of the evident connection between the atmosphere and the star. Another star not much less in brightness, and in the same field with the above, was perfectly free from any such appearance* (*d*).

This last object is so decisive in every particular, that we need not hesitate to admit it as a pattern, from which we are authorized to draw the following important consequences.

		h.	′	″		°	′
(*a*)	RA.	11	45	12.	PD.	33	43.
(*b*)		6	58	40.		91	29.
(*c*)		9	27	22.		30	11.
(*d*)		3	56	48.		59	50.

Supposing

Supposing the connection between the star and its surrounding nebulosity to be allowed, we argue, that one of the two following cases must necessarily be admitted. In the first place, if the nebulosity consist of stars that are very remote, which appear nebulous on account of the small angles their mutual distances subtend at the eye, whereby they will not only, as it were, run into one another, but also appear extremely faint and diluted; then, what must be the enormous size of the central point, which outshines all the rest in so superlative a degree as to admit of no comparison? In the next place, if the star be no bigger than common, how very small and compressed must be those other luminous points that are the occasion of the nebulosity which surrounds the central one? As, by the former supposition, the luminous central point must far exceed the standard of what we call a star, so, in the latter, the shining matter about the center will be much too small to come under the same denomination; we therefore either have a central body which is not a star, or have a star which is involved in a shining fluid, of a nature totally unknown to us.

I can adopt no other sentiment than the latter, since the probability is certainly not for the existence of so enormous a body as would be required to shine like a star of the 8th magnitude, at a distance sufficiently great to cause a vast system of stars to put on the appearance of a very diluted, milky nebulosity.

But what a field of novelty is here opened to our conceptions! A shining fluid, of a brightness sufficient to reach us from the remote regions of a star of the 8th, 9th, 10th, 11th, or 12th magnitude, and of an extent so considerable as to take up 3, 4, 5, or 6 minutes in diameter! Can we compare it to

the coruſcations of the electrical fluid in the aurora borealis? Or to the more magnificent cone of the zodiacal light as we ſee it in ſpring or autumn? The latter, notwithſtanding I have obſerved it to reach at leaſt 90 degrees from the ſun, is yet of ſo little extent and brightneſs as probably not to be perceived even by the inhabitants of Saturn or the Georgian planet, and muſt be utterly inviſible at the remoteneſs of the neareſt fixed ſtar.

More extenſive views may be derived from this proof of the exiſtence of a ſhining matter. Perhaps it has been too haſtily ſurmiſed that all milky nebuloſity, of which there is ſo much in the heavens, is owing to ſtarlight only. Theſe nebulous ſtars may ſerve as a clue to unravel other myſterious phænomena. If the ſhining fluid that ſurrounds them is not ſo eſſentially connected with theſe nebulous ſtars but that it can alſo exiſt without them, which ſeems to be ſufficiently probable, and will be examined hereafter, we may with great facility explain that very extenſive, teleſcopic nebuloſity, which, as I mentioned before, is expanded over more than ſixty degrees of the heavens, about the conſtellation of Orion; a luminous matter accounting much better for it than cluſtering ſtars at a diſtance. In this caſe we may alſo pretty nearly gueſs at its ſituation, which muſt commence ſomewhere about the range of the ſtars of the 7th magnitude, or a little farther from us, and extend unequally in ſome places perhaps to the regions of thoſe of the 9th, 10th, 11th, and 12th. The foundation for this ſurmiſe is, that, not unlikely, ſome of the ſtars that happen to be ſituated in a more condenſed part of it, or that perhaps by their own attraction draw together ſome quantity of this fluid greater than what they are intitled to by their ſituation in it, will, of courſe, aſſume the appearance of cloudy

ſtars; and many of thoſe I have named are either in this ſtratum of luminous matter, or very near it.

We have ſaid above, that in nebulous ſtars the exiſtence of the ſhining fluid does not ſeem to be ſo eſſentially connected with the central points that it might not alſo exiſt without them. For this opinion we may aſſign ſeveral reaſons. One of them is the great reſemblance between the chevelure of theſe ſtars and the diffuſed extenſive nebuloſity mentioned before, which renders it highly probable that they are of the ſame nature. Now, if this be admitted, the ſeparate exiſtence of the luminous matter, or its independance on a central ſtar, is fully proved. We may alſo judge, very confidently, that the light of this ſhining fluid is no kind of reflection from the ſtar in the center; for, as we have already obſerved, reflected light could never reach us at the great diſtance we are from ſuch objects. Beſides, how impenetrable would be an atmoſphere of a ſufficient denſity to reflect ſo great a quantity of light? And yet we obſerve, that the outward parts of the chevelure are nearly as bright as thoſe that are cloſe to the ſtar; ſo that this ſuppoſed atmoſphere ought to give no obſtruction to the paſſage of the central rays. If, therefore, this matter is ſelf-luminous, it ſeems more fit to produce a ſtar by its condenſation than to depend on the ſtar for its exiſtence.

Many other diffuſed nebuloſities, beſides that about the conſtellation of Orion, have been obſerved or ſuſpected; but ſome of them are probably very diſtant, and run out far into ſpace. For inſtance, about 5 minutes in time preceding ξ Cygni, I ſuſpect as much of it as covers near four ſquare degrees; and much about the ſame quantity 44′ preceding the 125 Tauri. A ſpace of almoſt 8 ſquare degrees, 6′ preceding α Trianguli, ſeems to be tinged with milky nebuloſity. Three

minutes

minutes preceding the 46 Eridani, ſtrong, milky nebuloſity is expanded over more than two ſquare degrees. 54′ preceding the 13th Canum venaticorum, and again 48′ preceding the ſame ſtar, I found the field of view affected with whitiſh nebuloſity throughout the whole breadth of the ſweep, which was 2° 39′. 4′ following the 57 Cygni, a conſiderable ſpace is filled with faint, milky nebuloſity, which is pretty bright in ſome places, and contains the 37th nebula of my Vth claſs, in the brighteſt part of it. In the neighbourhood of the 44th Piſcium, very faint nebuloſity appears to be diffuſed over more than 9 ſquare degrees of the heavens. Now, all theſe phænomena, as we have already ſeen, will admit of a much eaſier explanation by a luminous fluid than by ſtars at an immenſe diſtance.

The nature of planetary nebulæ, which has hitherto been involved in much darkneſs, may now be explained with ſome degree of ſatisfaction, ſince the uniform and very conſiderable brightneſs of their apparent diſk accords remarkably well with a much condenſed, luminous fluid; whereas to ſuppoſe them to conſiſt of cluſtering ſtars will not ſo completely account for the milkineſs or ſoft tint of their light, to produce which it would be required that the condenſation of the ſtars ſhould be carried to an almoſt inconceivable degree of accumulation. The ſurmiſe of the regeneration of ſtars, by means of planetary nebulæ, expreſſed in a former Paper, will become more probable, as all the luminous matter contained in one of them, when gathered together into a body of the ſize of a ſtar, would have nearly ſuch a quantity of light as we find the planetary nebulæ to give. To prove this experimentally, we may view them with a teleſcope that does not magnify ſufficiently to ſhew their extent, by which means we ſhall gather all their

light

light together into a point, when they will be found to assume the appearance of small stars; that is, of stars at the distance of those which we call of the 8th, 9th, or 10th magnitude. Indeed this idea is greatly supported by the discovery of a well defined, lucid point, resembling a star, in the center of one of them: for the argument which has been used, in the case of nebulous stars, to shew the probability of the existence of a luminous matter, which rested upon the disparity between a bright point and its surrounding shining fluid, may here be alledged with equal justice. If the point be a generating star, the further accumulation of the already much condensed, luminous matter, may complete it in time.

How far the light that is perpetually emitted from millions of suns may be concerned in this shining fluid, it might be presumptuous to attempt to determine; but, notwithstanding the unconceivable subtilty of the particles of light, when the number of the emitting bodies is almost infinitely great, and the time of the continual emission indefinitely long, the quantity of emitted particles may well become adequate to the constitution of a shining fluid, or luminous matter, provided a cause can be found that may retain them from flying off, or reunite them. But such a cause cannot be difficult to guess at, when we know that light is so easily reflected, refracted, inflected, and deflected; and that, in the immense range of its course, it must pass through innumerable systems, where it cannot but frequently meet with many obstacles to its rectilinear progression. Not to mention the great counteraction of the united attractive force of whole sidereal systems, which must be continually exerting their power upon the particles while they are endeavouring to fly off. However, we shall lay no stress upon a surmise of this kind, as the means of verifying

fying it are wanting: nor is it of any immediate conſequence to us to know the origin of the luminous matter. Let it ſuffice, that its exiſtence is rendered evident, by means of nebulous ſtars.

I hope it will be found, that in what has been ſaid I have not launched out into hypothetical reaſonings; and that facts have all along been kept ſufficiently in view. But, in order to give every one a fair opportunity to follow me in the reflections I have been led into, the place of every object from which I have argued has been purpoſely added, that the validity of what I have advanced might be put to the proof by thoſe who are inclined, and furniſhed, with the neceſſary inſtruments to undertake an attentive and repeated inſpection of the ſame phænomena.

W. HERSCHEL.

Slough, Jan. 1, 1791.

V. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon *in* Rutland; *by* Thomas Barker, *Eſq.*; *with the Rain in* Hampſhire *and* Surrey; *for the Year* 1789. *Communicated by* Thomas White, *Eſq. F.R.S.*

Read Feb. 17, 1791.

		Barometer.			Thermometer.						Rain.			
					In the Houſe.			Abroad.				Surrey.	Hampſhire.	
		Higheſt	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean	Lyndon	S. Lambeth.	Selbourn.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	30,25	28,00	29,23	47	27	36	47	13½	32	2,604	2,41	4,48	2,98
	Aftern.				50½	28	37	51½	21½	37				
Feb.	Morn.	29,79	28,13	29,18	47½	37½	42	46	31	37	1,847	2,51	4,11	3,31
	Aftern.				47½	39	43	51½	36½	44				
Mar.	Morn.	29,67	28,50	29,25	40½	34½	37½	37	22	32	1,152	2,32	2,47	2,30
	Aftern.				40½	36	38	46½	33	40				
Apr.	Morn.	29,70	28,61	29,28	53½	39½	46	51	32	41	1,010	1,24	1,81	1,58
	Aftern.				56½	41½	48	67	43	53				
May	Morn.	29,80	29,12	29,42	63	48	55½	59½	42½	50	1,677	2,80	4,05	4,03
	Aftern.				63½	49	57	71½	45½	63½				
June	Morn.	29,82	28,92	29,38	64	53½	58	62½	49	53	4,447	3,66	4,24	5,03
	Aftern.				66	55	59	77½	58	67				
July	Morn.	29,63	29,10	29,39	63½	56½	60½	62	49½	57	4,259	2,77	3,69	3,95
	Aftern.				65	58½	61½	78½	59½	69				
Aug.	Morn.	29,90	29,25	29,61	65	55	62	62½	50	57	0,331	1,91	0,99	0,33
	Aftern.				68	59½	63½	74½	60½	69				
Sept.	Morn.	29,88	28,85	29,40	63	52½	57½	57½	42	50½	2,846	1,87	2,82	3,58
	Aftern.				64	53½	59	72	55	63				
Oct.	Morn.	29,84	28,52	29,22	55½	43½	50	50	32	44	4,931	3,54	5,04	3,35
	Aftern.				57	43½	51	62	39	52				
Nov.	Morn.	29,90	28,25	29,26	44	38	42	43	30½	36½	1,199	——	3,67	1,69
	Aftern.				45	38½	42	50½	36½	43				
Dec.	Morn.	30,04	28,35	29,32	48	37½	43	50½	30½	40	1,699	1,51	4,63	3,48
	Aftern.				48½	38½	44	52½	34½	44				
											Inches 28.002		42,00	35,61

MANY have thought, that a hard winter generally comes after a wet ſummer, when the ground has been chilled with cold and rain; ſo the great froſt in 1740 came after a cold wet ſummer 1739; but it was not ſo in 1788, which was in general a dry ſummer, and in ſome places very much ſo, and not defective in hot weather. The whole year's rain was 17 inches, which is leſs than any year ſince 1750. The autumn was dry and fine, and ſo free from froſts, that ſeveral autumnal flowers were flowering in the garden when the froſt began, which are often cut off a month or more earlier.

The laſt day I have mentioned as mild was Nov. 22. and the firſt day of the froſt Nov. 26; it began in Hampſhire three days ſooner. But for more than the firſt fortnight it was very moderate, often freezing at night and thawing in the day; and it began to be ſevere Dec. 12.; was exceeding cold, and ſome very ſharp winds, and, as there was then no conſiderable ſnow, it entered deep into the ground. An imperfect thaw, Dec. 24. and 25. was followed by a great ſnow, and the froſt returned as hard as ever. There came ſnow again ſeveral times; and it lay ſo looſe and hard frozen on the ground, as to be often driving about into very great heaps; till January 13, 1789, when, in one of the worſt days of all for ſtorm, ſnow, and driving, it began to thaw at night; yet the ground was ſo hard frozen and cold, eſpecially where paths had been ſwept, that the moiſture of the air continued freezing for ſome days longer in cakes of ice on the bare ground and ſtones.

It was remarkable that this froſt was ſeverer ſouthward than northward, in France than England. It froze over our great rivers, yet not ſo as to venture to build on them, as was done in

in 1740. It alſo froze the much larger rivers of Europe, and was in moſt parts a ſevere winter; but to the north and north-weſt of us, it appears not to have been ſo, and that it was moderate in Scotland, and was chiefly great ſnows in Ireland.

The air was very ſharp during the froſt, and it froze exceeding hard, even within doors; yet I have ſeen the thermometer lower in other froſts than it was in this. The loweſt I ſaw was Jan. 12. at 13°¼; but in Jan. 1786, it was down at 11°¼; in the ſevere froſt Jan. 1776, it was 11° and 10°; Feb. 1784, at 9°; Dec. 1783, at 8°¼; and one morning, Feb. 12, 1771, it was down at 4°, which is the loweſt I ever ſaw it.

The froſt from Dec. 1739 to Feb. 1740 was the greateſt I ever knew, and many trees, ſhrubs, and plants, were killed by it, or nearly ſo; but in general they eſcaped this winter. In moſt gardens every plant of roſemary was killed that year, few or none were much hurt this winter. In 1740, the wheat in many countries received great damage; in this froſt I believe it was very little hurt. Moſt of the branches of common furze were killed in 1740, many quite down to the ground, and ſome were entirely deſtroyed; and I have known many ſuffer much, and ſome killed, in 1776, and other hard winters; but this year only a few were much hurt. Many turneps were frozen in the fields this year, a huſbandry not ſo much practiſed in 1740; and many apples and pears were frozen in the houſe, where they were not carefully preſerved; yet, I think, there was leſs of this than was apprehended. Artichokes, I think, ſuffered as much as moſt things in the garden; ſome were killed, and others ſo hurt as to prevent their bearing, but many eſcaped; but there was a greater loſs among them in 1740.

Many walnut-trees were ſplit from the collar to the root by the great froſt in 1740, ſo that a knife might be thruſt in eight or ten inches; the clefts cloſed again in ſummer, but never united. They grew out into a ſeam higher than the reſt of the wood, and have ſo continued ever ſince, yet without hurt either to the growth or bearing of the trees, and ſeveral of them were again ſplit by the late froſt. Such ſeams may be ſeen on many walnut-trees where the cauſe is not remembered. It ſeems odd, that clefts which did not affect the growth of a tree ſhould yet never heal, but remain an indelible mark for ſo long a time; but it ſeems to me, that if wood is once parted, it will never join again, for the whole growth of a tree is between the bark and the wood; but the cleft may be covered over with new wood, as we ſometimes ſee a branch broken off when the tree is young covered over with a great thickneſs of timber. I have known ſeveral aſh trees ſplit by lightning without a twig being killed; but, in ſeveral years they ſtood afterward, there were no ſigns of their uniting again. In an oak, which had ſome bark ſtruck off by the ſame means laſt year, but is not ſplit that I know of, ſome of the leſſer branches withered.

One thing ſeems to have been more common this year than in 1740, and that was the loſs of fiſh in ponds. Where the ponds were deep, well ſupplied with water, and the ice unbroken, no fiſh died; but where the water was ſhallow, little or no current, and the ice kept broken, many periſhed; and in ſome places, where all theſe cauſes concurred, they were all killed. The difference might ariſe from the want of water this year after a dry autumn, of which there was no defect in 1739. Carp were taken out of a pond where the ice was broken, frozen crooked and ſtiff without the leaſt motion, and

ice

ice hanging about them; but, being laid on dry ſtraw in a cellar, they all recovered. Some have made a practice of breaking the ice to give the fiſh air; but, by all the examples I heard of in this froſt, it appears to be a great miſtake, and that they are much ſafer without it.

Times of diſtreſs will make creatures look out for unuſual food. A land animal does not ſeem naturally to live on fiſh; but in this froſt a fox was frequently tracked to the mouth of a covered drain, juſt deep enough to let in the fiſh from a pond, and was one morning ſeen eating one on the bank, the blood of which was traced on the ſnow to the drain, ſhewing plainly that it came from thence.

After the froſt broke it was windy and wet, and the air did not become mild for ten days or a fortnight; and the ground was ſo dry within, that the melting of the ſnow and the rain together did not make great floods. It continued often windy and ſhowery till March; but from the latter end of January it was, as to warmth, mild. With March the winter returned, there was almoſt conſtant north winds, froſty mornings or quite froſt, and ſometimes hard and frequent ſnow. This ſtopped the ſeed time, which was begun before, and made it late; but when it did come, it was good. The ſpring was backward, and frequent froſty mornings; but mended gradually, and things came on, though ſlowly, and the ſpring was dry till the middle of May.

Then came a ſhowery and fine growing time for three weeks, and after a ſhort time it became hot and dry. The middle of June a wet ſeaſon began with a very great rain, and it was wet without interruption till the end of July. There was a great deal of hay made this year, but little of it right good. Many of the meadows were flooded; the uplands could not be well gotten for

the wet; there were great eddiſhes, and a vaſt deal of late graſs, much of which was well made into hay in Auguſt, which was a fine dry ſummer month; but ſuch late graſs is not ſo good as the earlier. This month was rather an interruption than a ceaſing of the wet ſeaſon, which began again the beginning of September, and continued to the end of the year, but the rain fell in leſs quantity from the middle of October to the middle of December; but the ſeaſon has been very open and mild, ſcarcely any froſt, and the ground ſtill green at Chriſtmas.

The ſummer was ſo wet there was very little honey this year. The growth of trees was very great; many ſhoots were three and four feet long or more; and a young aſh tree, of ſix feet high, in the garden, made a ſhoot five feet and an half long, and as thick as a finger. The grain was very rank and foul; there was bulk enough upon the ground, but it yielded very badly to the threſher, perhaps not greatly amiſs to the acre. Harveſt being late, but little was got in during the fine month of Auguſt: the white corn was however carried in tolerable order; but a great many of the beans and peaſe were ſpoiled.

VI. *Observations on certain horny Excrescences of the Human Body.* By Everard Home, *Esq. F. R. S.*

Read February 17, 1791.

THE history of diseases belongs not properly to the province of the naturalist or philosopher; it is intimately connected with the inquiries of the physician and anatomist; but when disease becomes a cause of the formation of parts similar to others existing in nature, but rendered uncommon by novelty of situation, or produced in animals to which they are not naturally appropriated, it may be considered as having instituted a monstrous variety, highly deserving of attention from the naturalist.

To describe such varieties is indeed more fully the office of natural history than of medicine; but the investigation of diseases which are found to subvert the ordinary laws of nature respecting the situation or production of parts in an animal body, undoubtedly belongs to the medical practitioner.

By these considerations I have been induced to lay before the Royal Society the following account of a disease which occurs sometimes in the human body, very remarkable in its effects, but very little understood as to its cause; namely, the production of an excrescence similar to a horn. So curious a phænomenon has naturally attracted the attention of the ignorant as well as the philosopher; and the individuals who have had the misfortune to be subject to this disease have been considered as monsters.

Horny excrescences arising from the human head have not only occurred in this country, but have been met with in several

veral other parts of Europe; and the horns themſelves have been depoſited as valuable curioſities in the firſt collections in Europe.

In giving the hiſtory of a diſeaſe ſo rare in its occurrence, and in its effects ſo remarkable as almoſt to exceed belief, it might be thought right to take ſome pains in bringing proofs to aſcertain that ſuch a diſeaſe does really exiſt: I conſider the doing ſo as leſs neceſſary at preſent, there being two women now alive, and reſiding in England, who are affected by the complaint. I ſhall, however, in the courſe of this Paper, bring other evidence from the teſtimony of the moſt reſpectable authors who have conſidered this ſubject.

The two following caſes contain a very accurate and diſtinct hiſtory of the progreſs of the diſeaſe through its different ſtages, and make any further detail of the ſymptoms intirely unneceſſary.

Mrs. LONSDALE, a woman 56 years old, a native of Horncaſtle in Lincolnſhire, fourteen years ago, obſerved a moveable tumor on the left ſide of her head, about two inches above the upper arch of the left ear, which gradually increaſed in the courſe of four or five years to the ſize of a pullet's egg, when it burſt, and for a week continued to diſcharge a thick, gritty fluid. In the center of the tumor, after the fluid was diſcharged, ſhe perceived a ſmall ſoft ſubſtance, of the ſize of a pea, and of a reddiſh colour on the top, which at that time ſhe took for proud fleſh. It gradually increaſed in length and thickneſs, and continued pliable for about three months, when it firſt began to put on a horny appearance. In two years and three months from its firſt formation, made deſperate by the increaſed violence of the pain, ſhe attempted to tear it from her head; and with much difficulty, and many efforts, at length broke it in the middle, and afterwards tore the root from her head, leaving a conſiderable depreſſion which ſtill remains in

the

the part where it grew. Its length altogether is about five inches, and its circumference at the two ends about one inch; but in the middle rather leſs. It is *curled* like a ram's horn contorted, and in colour much reſembling iſinglaſs.

From the lower edge of the depreſſion another horn is now growing, of the ſame colour with the former, in length about three inches, and nearly the thickneſs of a ſmall gooſe quill; it is leſs contorted, and lies cloſe upon the head.

A third horn, ſituated about the upper part of the lambdoidal ſuture, is much curved, above an inch in length, and more in circumference at its root: its direction is backwards, with ſome elevation from the head. At this place two or three ſucceſſive horns have been produced, which ſhe has conſtantly torn away; but, as freſh ones have ſpeedily followed, ſhe leaves the preſent one unmoleſted in hopes of its dropping off.

Beſides theſe horny excreſcences, there are two tumors, each the ſize of a large cockle; one upon the upper part, the other about the middle of the left ſide of the head; both of them admit of conſiderable motion, and ſeem to contain fluids of unequal conſiſtence; the upper one affording an obſcure fluctuation, the other a very evident one.

The four horns were all preceded by the ſame kind of incyſted tumours, and the fluid in all of them was gritty; the openings from which the matter iſſued were very ſmall, the cyſts collapſed and dried up, leaving the ſubſtance from which the horn proceeded diſtinguiſhable at the bottom. Theſe cyſts gave little pain till the horns began to ſhoot, and then became very diſtreſſing, and continued with ſhort intervals till they were removed. This caſe is drawn up by the ſurgeon who attended the woman for many years, which gave him frequent

opportunities of ſeeing the diſeaſe in its different ſtages, and acquiring an accurate hiſtory of its ſymptoms.

Mrs. ALLEN, a middle-aged woman, reſident in Leiceſterſhire, had an incyſted tumor upon her head, immediately under the ſcalp, very moveable, and evidently containing a fluid. It gave no pain unleſs preſſed upon, and grew to the ſize of a ſmall hen's egg. A few years ago it burſt, and diſcharged a fluid; this diminiſhed in quantity, and in a ſhort time a horny excreſcence, ſimilar to thoſe above mentioned, grew out from the orifice, which has continued to increaſe in ſize; and in the month of November 1790, the time I ſaw it, was about five inches long, and a little more than an inch in circumference at its baſe. It was a good deal contorted, and the ſurface very irregular, having a laminated appearance. It moved readily with the ſcalp, and ſeemed to give no pain upon motion; but, when much handled, the ſurrounding ſkin became inflamed. This woman came to London, and exhibited herſelf as a ſhow for money; and it is highly probable, that ſo rare an occurrence would have ſufficiently excited the public attention to have made it anſwer her expectations in point of emolument, had not the circumſtance been made known to her neighbours in the country, who were much diſſatisfied with the meaſure, and by their importunity obliged her huſband to take her into the country.

That the caſes which I have related may not be conſidered as peculiar inſtances from which no concluſions can be drawn, it may not be amiſs to take notice of ſome of the moſt remarkable hiſtories of this kind, mentioned by authors, and ſee how far they agree with thoſe I have ſtated, in the general characters that are ſufficiently obvious to ſtrike a common obſerver; for the vague and indefinite terms in which authors expreſs themſelves

ſelves on this ſubject ſhew plainly, that they did not underſtand the nature of the diſeaſe, and their accounts of it are not very ſatisfactory to their readers.

In the *Ephemerides Academiæ Naturæ Curioſorum* there are two caſes of horns growing from the human body. One of theſe inſtances was a German woman *, who had ſeveral ſwellings, or ganglions, upon different parts of her head, from one of which a horn grew. The other was a nobleman †, who had a ſmall tumor, about the ſize of a nut, growing upon the parts covering the two laſt or lowermoſt vertebræ of the back. It continued for ten years, without undergoing any apparent change; but afterwards enlarged in ſize, and a horny excreſcence grew out from it.

In the Hiſtory of the Royal Society of Medicine ‡, there is an account of a woman, 97 years old, who had ſeveral tumors on her head, which had been 14 years in growing to the ſtate they were in at that time: ſhe had alſo a horn which had originated from a ſimilar tumor. The horn was very moveable, being attached to the ſcalp, without any adheſion to the ſcull. It was ſawn off, but grew again, and although the operation was repeated ſeveral times, the horn always returned.

Bartholine, in his Epiſtles §, takes notice of a woman who had a tumor under the ſcalp, covering the temporal muſcle. This gradually enlarged, and a horn grew from it, which had become twelve inches long in the year 1646, the time he ſaw it. He gives us a repreſentation of it, which bears a very accurate reſemblance to that which I have mentioned to have ſeen in November 1790. No tumor or ſwelling is

* Ephem. Acad. Nat. Cur. Dec. iii. An. V. Append. p. 148.

† Ibid. Dec. i. An. I. Obſervat. 30.

‡ Hiſtoire de la Société Royale de Médecine, 1776, p. 316.

§ Epiſtol. Thom. Barthol.

expreſſed in the figure; but the horn is coming directly out from the ſurface of the ſkin.

In the Natural Hiſtory of Cheſhire *, a woman is mentioned to have lived in the year 1668, who had a tumor or wen upon her head for 32 years, which afterwards enlarged, and two horns grew out of it; ſhe was then 72 years old.

There is a horny excreſcence in the Britiſh Muſeum, which is eleven inches long, and two inches and a half in circumference at the baſe, or thickeſt part. The following account of this horn I have been favoured with by Dr. GRAY, taken from the records of the Muſeum. A woman, named FRENCH, who lived near Tenterden, had a tumor or wen upon her head, which increaſed to the ſize of a walnut; and in the 48th year of her age this horn began to grow, and in four years arrived at its preſent ſize †.

There are many ſimilar hiſtories of theſe horny excreſcences in the authors I have quoted, and in ſeveral others; but thoſe mentioned above are the moſt accurate and particular with reſpect to their growth, and in all of them we find the origin was from a tumor, as in the two caſes I have related; and although the nature of the tumor is not particularly mentioned, there can be no doubt of its being of the incyſted kind, ſince in its

* LEE's Natural Hiſtory of Lancaſhire and Cheſhire.

† The following extract is taken from the Minutes of the Royal Society, Feb. 14, 1704-5.

"A Letter was read from Dr. CHARIERE, at Barnſtaple, concerning a horn, "ſeven inches long, cut off the ſecond vertebra of the neck of a woman in that "neighbourhood.

"Dr. GREGORY ſaid, that one of ſeven inches long, and of a dark brown "colour, was cut off from a woman's temple at Edinburgh.

"Dr. NORRIS ſaid, that two horns had been cut off from a woman's head "in Cheſhire.

progreſs

progreſs it exactly reſembled them, remaining ſtationary for a long time, and then coming forwards to the ſkin; and the horn being much ſmaller than the tumor previouſly to the formation of the horn, is a proof that the tumor muſt have burſt, and diſcharged its contents.

From the foregoing account it muſt appear evident, that theſe horny excreſcences are not to be ranked among the appearances called *luſus naturæ*: nor are they altogether the product of diſeaſe, although undoubtedly the conſequence of a local diſeaſe having previouſly exiſted; they are, more properly ſpeaking, the reſult of certain operations in the part for its own reſtoration; but the actions of the animal œconomy being unable to bring them back to their original ſtate, this ſpecies of excreſcence is formed as a ſubſtitute for the natural cuticular covering.

To explain the manner in which theſe horns are formed, it will be neceſſary to conſider the nature of incyſted tumors a little more fully; and in doing ſo we ſhall find, that this particular ſpecies does not differ in its principle, nor materially in its effects, from many others which are not uncommonly met with in the human body, as well as in thoſe of many other animals, which, as they are more frequent in their occurrence, are alſo much better underſtood.

Incyſted tumors differ exceedingly among themſelves, both in the nature of their contents, and in their progreſs towards the external ſurface of the body. Many of them have no reference to our preſent purpoſe; it is only the more indolent kind to which I mean now to advert: ſome of theſe, when examined, are not found to contain a fluid, but a ſmall quantity of thick, curd-like matter, mixed with cuticle broken down into ſmall parts, and upon expoſing the internal ſurface of the cyſt, it is found

found to have an uniform cuticular covering adhering to it, similar to that of the cutis on the surface of the body, from which it only differs in being thinner, and more delicate, bearing a greater resemblance to that which covers the lips. Others of this kind, instead of having cuticle for their contents, are filled with hair mixed with a curdled substance, or hair without any admixture whatever, and have a similar kind of hair growing upon their internal surface, which is likewise covered with a cuticle. These cuticular incysted tumors were, I believe, first accurately examined by Mr. HUNTER, to whom we are likewise indebted for an explanation of the mode in which the parts acquire this particular structure.

Mr. HUNTER considers the internal surface of the cyst to be so circumstanced respecting the body, as to lose the stimulus of being an internal part, and receive the same impression from its contents, either from their nature, or the length of application, as the surface of the skin does from its external situation. It therefore takes on actions suited to such stimuli, undergoes a change in its structure, and acquires a disposition similar to the cutis, and is consequently possessed of the power of producing cuticle and hair. What the mode of action is, by which this change is brought about, is not easily determined; but from the indolence of these complaints, it most probably requires a considerable length of time to produce it. That the lining of the cyst really does possess powers similar to cutis, is proved by the following circumstances: that it has a power of forming a succession of cuticles like the common skin; and what is thrown off in this way is found in the cavity of the cyst. It has a similar power respecting hair, and sometimes the cavity is filled with it, so great a quantity has been shed by the internal surface. Besides these circumstances,

the hair found in the cyſt correſponds in appearance with that which grows upon the body of the animal; and when incyſted tumors of this kind form in ſheep, they contain wool. What is ſtill more curious, when ſuch cyſts are laid open, the internal ſurface undergoes no change from expoſure, the cut edges cicatrize, and the bottom of the bag remains ever after an external ſurface. Different ſpecimens, illuſtrative of the above-mentioned circumſtances, are preſerved in Mr. HUNTER's collection of diſeaſes.

The cyſts that produce horny excreſcences (which are only another modification of cuticle) are very improperly conſidered as giving riſe to horns; for if we examine the mode in which this ſubſtance grows, we ſhall find it the ſame with the human nails, coming directly out from the ſurface of the cutis. It differs from the nails in not being ſet upon the ſkin by a thin edge, but by a ſurface of ſome breadth, with a hollow in the middle, exactly in the ſame manner as the horn of the rhinoceros*; at leaſt this is evidently the caſe in the ſpecimen preſerved in the Britiſh Muſeum, and in one which grew out from the tip of a ſheep's ear; they are alſo ſolid, or nearly ſo, in their ſubſtance.

This mode of growth is very different from that of horns, which are all formed upon a core, either of bone or ſoft parts, by which means they have a cavity in them; a ſtructure peculiar to this kind of cuticular ſubſtance.

Incyſted tumors in different animals would appear, from theſe obſervations, to be confined in their production to the

* The horn of the Rhinoceros is a cuticular appendage to the ſkin, ſimilar to nails and other cuticular excreſcences, being in no reſpect allied to horns but in the external appearance.

cuticular

cuticular ſubſtance proper to the animal in which they take place; for, although cuticle, hair, nail, hoof, and horn, are equally productions of animal ſubſtance, only differing in trivial circumſtances from each other, we do not find in the human ſubject any inſtance of an incyſted tumor containing a ſubſtance different from the cuticle, hair, and nails of the human body, to which laſt the horny excreſcences, the ſubject of the preſent Paper, are certainly very cloſely allied, both in growth, ſtructure, and external appearance; and when of ſome length, they are found to be ſo brittle as to break in two, upon being roughly handled, which could not happen either to hoof or horn. In the ſheep they produce wool inſtead of hair; and in one inſtance in that animal, where they gave riſe to an horny excreſcence, it was leſs compact in its texture, and leſs brittle than ſimilar appearances in the human ſubject; upon being divided longitudinally, the cut ſurface had more the appearance of hoof, and was more varied in its colour than nail.

Incyſted tumors being capable of producing horns, upon the principle we have laid down, is contrary to the uſual operations of nature; for horns are not a production from the cutis, and although not always formed upon a bony core, but frequently upon a ſoft pulp, that ſubſtance differs from common cutis in its appearance, and extends a conſideraway into the horn: it is probable, that this pulp requires a particular proceſs for its formation *.

* A ſheep, about four years old, had a large horn, three feet long, growing upon its flank. It had no connection with bone, and appeared to be only attached to the external ſkin. It dropped off in conſequence of its weight having produced ulceration in the ſoft parts to which it adhered. Upon examining it there was a fleſhy ſubſtance, ſeven inches long, of a fibrous texture filling up its cavity upon which the horn had been formed.

I ſhall conclude this Paper by obſerving, that the caſes of horns, as they are commonly termed, upon the human head, are no more than cuticular productions ariſing from a cyſt, which in its nature is a variety of thoſe tumors deſcribed by Mr. HUNTER under the general name of cuticular incyſted tumors *.

Theſe incyſted tumors, when conſidered as varieties of the ſame diſeaſe, form a very complete and beautiful ſeries of the different modes by which the powers of the animal œconomy produce a ſubſtitute for the common cuticle upon parts which have been ſo much affected by diſeaſe as to be unable to reſtore themſelves to a natural ſtate.

* The principle upon which the production of theſe excreſcences depends being once explained, the modes of preventing their formation, and removing them when formed, will be readily underſtood, the deſtruction of the cyſt being all that is required for that purpoſe. This may be done before the tumor opens externally, or even after the excreſcence has begun to ſhoot out, and will be better effected by diſſection than eſcharotics, ſince the ſucceſs of the operation depends upon the whole of the bag being removed.

VII. *Conſiderations on the Convenience of meaſuring an Arch of the Meridian, and of the Parallel of Longitude, having the Obſervatory of* Geneva *for their common Interſection. By* Mark Auguſtus Pictet, *Profeſſor of Philoſophy in the Academy of* Geneva; *in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read February 24, 1791.

SIR,

THE accurate knowledge of the dimenſions and true figure of the earth is not a matter of mere curioſity. Aſtronomy and navigation are ſo cloſely connected with it, that the philoſophers of the preſent century have purſued this enquiry through the moſt diſcouraging difficulties; and governments themſelves have contributed conſiderable ſums towards its ſucceſs.

We know that, notwithſtanding theſe efforts, the end is not yet obtained. There are five different concluſions on this ſubject; one of which is given by Sir Isaac Newton's theory; the others are the reſult of four different meaſurements, which appear the moſt creditable among thoſe that have been performed. The extremes give $\frac{1}{111}$ and $\frac{1}{304}$ for the difference between the polar and equatorial diameters of the earth, that is, two fractions, one of which is more than double the other. The cauſe of theſe diſagreements is yet unknown; perhaps the figure of the earth is really irregular; perhaps the ſeveral meaſure-

meaſurements have not been executed with the very minute exactneſs requiſite in ſo nice and ſo important an undertaking.

The liberal and well-conducted operations carried on by the Royal Society, under the direction of the late General ROY, for the trigonometrical determination of the diſtance between the Obſervatories of Greenwich and Paris, render this laſt ſuppoſition extremely probable. It now ſeems evident, that the ſubſtances employed before for the actual meaſurement of the baſes muſt have been influenced in their length by pyrometrical and hygrometrical effects, which were either unknown or ill-eſtimated at that time. The inſtruments alſo for obſerving the celeſtial and terreſtrial angles were far from the perfection to which they have ſince been brought. In a word, the whole of the work ſhould be again undertaken with the far greater degree of accuracy which is now within our reach.

Struck with the importance of theſe facts, I take the liberty of tranſmitting to you, Sir, for the conſideration of the Royal Society, the preſent plan for meaſuring, by a commiſſion of its Members, an arch of the meridian, and of a parallel of longitude, having the Obſervatory of Geneva for their common point of interſection. Frequent excurſions in our neighbouring mountains have convinced me, not only that the meaſurement could be made, but that it would be perhaps the moſt eaſily executed of any hitherto attempted. The rough ſketch, with its explanation, joined to the preſent Paper, will give ſome account of the particulars which I have been able to collect relative thereto. At preſent I ſhall only lay before the Society a general ſurvey of the undertaking.

The beſt maps place the town of *St. Jean de Maurienne* nearly ſouth of Geneva, at the diſtance of about 58 minutes of latitude. It would be impoſſible to extend the meaſurement

farther ſouthwards, the central and inacceſſible chain of the Alps being in the way; but if a greater arch ſhould be deſired, it might be eaſily protracted about 26 minutes north of Geneva.

I lately made an excurſion to St. Jean de Maurienne, to examine whether the poſition of the town would be convenient in other reſpects; and if, in particular, the lateral attraction of ſome neighbouring mountain was not to be feared. It appeared to me, that the place being ſurrounded by mountains of nearly equal maſſes, and ſituated at almoſt equal diſtances, their effects would be hardly perceptible; and, ſuppoſing there ſhould remain any doubt about their influence, this influence might be eaſily aſcertained by zenith diſtances, obſerved at the two extremities of a little plain in which the town is built, and compared with the real diſtance of the ſtations, determined by an actual meaſurement. That town being the reſidence of a biſhop, and containing near 3000 inhabitants, might furniſh the obſervers with a convenient building for the zenith ſector, and the occaſional help and neceſſaries which might be required. The great poſt-road from hence into Italy, over Mount Cenis, paſſing through it, is alſo an advantageous circumſtance.

The diſpoſition and bearing of the vallies from that town, which would be the ſouthernmoſt extremity of the arch, is advantageous for the ſeries of triangles: for I have ſeen from the top of a mountain near St. Jean, called *Le Mont Sapey*, two parallel chains extending to the north on both ſides of the river *Arc*, and there appeared to be in their ſummits a great choice for convenient ſtations, as far as the confluence of the *Arc* and the river *Iſère* near *Aiguebelle*, from whence the mountains in the parallel of *Chamberi* are all viſible. From this laſt parallel to Geneva, and farther, there are not only no difficulties, but the ſtations are for the greateſt part already determined.

mined. The mountains and lines drawn in black, on the map ſubjoined (Tab. II.), ſhew the ſtations and triangles already obſerved; thoſe in red * are only preſumed, but their places will probably differ little in reality from thoſe which are there indicated. The map, as to the ſituation of mountains, has no pretenſions but to ſuch an exactneſs as may reſult from angles obſerved with a HADLEY's ſextant, without a reduction to the horizon; but the ſituation and figure of our lake is exactly true, being the reſult of a minute and accurate ſurvey taken by the late Profeſſor MALLET and myſelf.

The viſible part of the meridian of Geneva is ſoon terminated northwards by the firſt chain of Mount Jura; but the country opens to the N.N.E. and the northern ſtation might be eaſily choſen in ſome place of the *Pays de Vaud*, viſible from the Obſervatory of Geneva, and which could be determined by only one additional triangle. I take the liberty to point out two ſuch places. The one, called *Vincy*, about 16 minutes north of Geneva, and where the circumſtance of my poſſeſſing a country houſe would facilitate the diſpoſitions neceſſary for the aſtronomical obſervations. The other place is the top of a mountain, called the *Dent de Vaulion*, making part of the chain of Mount Jura, and where an occaſional obſervatory might be erected without much difficulty: it is 10 minutes north of Vincy, or 26′ of Geneva. The whole arch from *St. Jean de Maurienne* to this laſt place would be about 1 degree 24 minutes.

The celeſtial obſervations might perhaps be made in the four places above mentioned; and the meridian arch would be thus obtained in three portions, whoſe compariſon with the terreſtrial ſections, meaſured geometrically, would be a proof of the accuracy of the operation.

* In the engraving of this map, dotted lines are employed, to expreſs whatever was drawn with red in the original.

It

It is obſervable, on the inſpection of the map, that the chain of triangles lies moſtly on the weſt ſide of the meridian line. I was forced to diſpoſe it thus, to be within reach of the only place where a convenient baſis could be found, *viz.* the marſhes of *Choutagne*. I have ſeen this place, and am well aſſured, that the meaſurement in a convenient ſeaſon would be attended with no difficulty. The diſpoſition and proportion of the baſis of about 29,000 feet traced on the map, are the reſult of operations I have made on the ſpot.

The ſouthern part of the meridian line, viſible from the Obſervatory of Geneva, paſſes over the ſummit of a mountain called *Mount Salève*, where we have a meridian mark, at the diſtance of about 5600 toiſes, and at the height of about 500 toiſes above the level of the lake. I have obſerved from that ſummit, that the ſame line protracted ſouthwards is not intercepted by the mountains but at a great diſtance, and in a place which, as far as I could judge, muſt be near the ſouthern end of the arch. I am ignorant if that place is acceſſible; but if it ſhould happen to be ſo, it would be a fortunate circumſtance, inaſmuch as it would offer a very ſimple, quick, and accurate verification of the direction of the meridian line reſulting from the chain of triangles, by actually protracting the viſual line given immediately by the tranſit inſtrument of the Obſervatory down to the end of the arch, by the help of two intermediate ſtations only. I do not believe that ſuch a verification has ever yet been practiſed.

We ſee hitherto no local difficulties in the meaſurement of an arch of about 84 minutes of the meridian of Geneva. The meaſurement of the parallel of longitude, eaſtwards of the ſame place, ſeems to be of a ſtill eaſier execution, inſomuch that I

believe there are few places on earth better diſpoſed for the operation.

The Republic of *Vallais* in Switzerland offers an extenſive, broad, and nearly ſtraight valley, bordered on both ſides by high mountains. It is ſituated about the parallel of Geneva, runs eaſtward for many leagues from the town of *Martigny*, and to the weſtward is ſeparated from the mountains of *Chablais* in Savoy, by a very lofty chain, in which there is an acceſſible ſummit, called *The Glacier de Büet*, or *La Mortine*.

This mountain is placed, as by a miracle, in ſuch a poſition as to be viſible from the Obſervatory of Geneva, and about 10 minutes weſt of it, as alſo from almoſt every elevated ſituation in the *Haut Vallais*. Its ſummit is acceſſible by a much eaſier aſcent than that which was diſcovered by Meſſ. DE LUC; and a ſignal made there by the Indian lights would be viſible eaſt and weſt along the parallel, to the whole diſtance of perhaps two degrees between the two extreme ſtations; for as the obſervations relative to the regulating of the clocks do not require any conſiderable apparatus, they could be performed in the moſt diſtant hamlets from which the ſignal ſhould be viſible.

As to the trigonometrical meaſurement along the parallel, one may conclude, from the triangles delineated on the map, that it might be executed with a ſmaller number of operations than that of the meridian arch. I was aſſiſted in the deſignation of the probable ſtations in the *Vallais* by my ingenious friend Mr. WILD, Director of the Salt Works and Mines of the Republic of Bern, and reſiding at *Bex*; from whence, by frequent excurſions, he has acquired a ſufficient knowledge of the names and poſitions of the neighbouring mountains to enable me to complete the ſketch in the eaſtern part of the parallel.

Should

Should the method, propoſed by the late General ROY *, for aſcertaining the length of the parallel independently of aſtronomical obſervations, be adopted, it might be carried into execution with no great difficulty from the ſummit of the ſame mountain, where we juſt now ſuppoſed the ſignal by the Indian lights to be placed.

The triangles relative to the meaſurement of the parallel make but one ſuite with thoſe of the meridian, and there are four very convenient places along the ſame parallel for meaſuring baſes of verification. They are perfectly level plains, forming the bottom of the valley through which the Rhone flows between the towns of *Aigle* and *Villeneuve*, and between *Martigny* and *Sion*. Their relative ſituations are roughly indicated in the map. The above general conſiderations, together with the particulars which are ſubjoined to the ſketch, ſeem to aſcertain the full practicability of the enterprize. May I be allowed to add a few reflections on its conveniency?

The re-union of the two meaſurements (of latitude and longitude) in the ſame ſpot, is an advantageous circumſtance; and the more ſo, if we conſider that this ſpot lies between the 45 and 46th degree, that is, in the mean latitude between the pole and equator, near which latitude the mean radius of the earth takes place in the well-founded ſuppoſition of its being a ſpheroid. This radius, found by the moſt accurate meaſurement hitherto attempted, would become a ſtandard, and to which the reſults of the equatorial and northern meaſurements being compared, the true figure of the earth would be the better aſcertained.

The poſition or relative longitude of the meridian of Geneva iswell determined by a great number of obſerved immerſions

* Phil. Tranſ. 1787, p. 216. et ſeq.

and emersions of the satellites of Jupiter, and by some occultations of stars by the moon. These observations were performed by the late Professor J. A. MALLET, Mr. J. TREMBLEY, and myself. The greater part of them are already calculated, and their mean result must be near the truth. The latitude of the same place would be ascertained by the celestial observations essential to the measurement, and would verify our determination taken from the observed culmination of zenith stars. Lastly, the height of Geneva above the level of the Mediterranean Sea (which is supposed to be about 196 toises) can be obtained with a sufficient precision from the barometrical observations which for several years past have been regularly made here.

The Observatory of Geneva, in which, since the death of my late friend Professor MALLET, I have continued a series of observations, would be at the free disposition of the Commissioners whom the Royal Society should think proper to entrust with the whole work. It is furnished with the following instruments, and consequently there would be no occasion for sending those of the same sort from England.

1°, A very good transit instrument of 5 feet, made by SISSON. The telescope is achromatic, magnifying about 70 times.

2°, A quadrant of 2½ feet radius with two achromatic telescopes and a micrometer screw. The quadrant is moveable on an azimuthal circle. The whole made by the same artist.

3°, Two astronomical clocks, with gridiron pendulums. One remarkably good, made by the late SHELTON; the other by LE PAUTE. This last might be carried to any of the stations.

4°, An achromatic telescope of 10 feet, made by DOLLOND, with three magnifying powers.

5°, A 3½ feet achromatic, with great aperture, and four magnifying powers, made alſo by DOLLOND, mounted on a parallactic ſtand, and carrying occaſionally an ocular micrometer*.

6°, A very good RAMSDEN's 30-inches achromatic refractor with four powers; beſides ſome other teleſcopes and inſtruments of leſs importance.

The capital inſtruments which, I ſuppoſe, it would be neceſſary to ſend from England, would be one or two zenith ſectors, one or two clocks, and the inſtrument for obſerving the terreſtrial angles, the accuracy of which is of the utmoſt importance. I ſaw in London, three years ago, that made by Mr. RAMSDEN, and employed ſo ſucceſsfully in the late meaſurements from Hounſlow Heath to the French Coaſt. It is a matter of doubt to me, whether this precious inſtrument would be allowed to go out of the kingdom; and even ſhould that be permitted, as far as I can judge from the impreſſion it has left on my memory, it would be too heavy and of too large a bulk to be conveniently carried to the top of mountains. The inſtrument ought perhaps to be made of ſeparable parts eaſily put together, and capable of being ſteadily adjuſted: they might be packed ſeparately in a box, not exceeding the common charge of a man. The boxes could be ſo contrived as to fill a larger one, ſuſpended on ſprings in a ſmall narrow carriage made on purpoſe, and in which all the neceſſary things beſides could be collected. Such a diſpoſition would be very convenient, not only for ſending the inſtruments ſafe from England, but alſo to carry them from one ſtation to another through croſs country roads, with more quickneſs and leſs

* We are indebted for the poſſeſſion of that valuable inſtrument to the munificence of my learned friend M. DE SAUSSURE, who gave it to M. TREMBLEY, to whom it now belongs.

trouble and danger. A convenient tent, to ſhelter the inſtruments in the ſtations, ſhould be added, or might be made at Geneva.

The glaſs rods for the meaſurement of the baſis could be drawn of any convenient ſhape, in a glaſs-houſe not far diſtant from Geneva. The thermometers, and any additions or repairs the apparatus might require, would be duly performed by Meſſ. Paul, eminent artiſts of this town; and we abound with artiſts of inferior merit.

Thus far, Sir, I foreſee no capital obſtacles to the execution of the plan I have the honour of laying before the Royal Society. All the *data* ſeem in favour of it; and I may add, as another favourable circumſtance, that the ſtar *Capella* of the firſt magnitude culminates between the zeniths of *Geneva* and *St. Jean Maurienne*, and conſequently might be obſerved ſimultaneouſly from both ſtations at any hour in which the ſeaſon of the year would bring its meridian tranſit.

I ſhall conclude this Paper with a few more reflections on the undertaking in general.

Suppoſing it to be the intention of the Royal Society to attempt the inveſtigation of the true figure of the earth, by meaſurements executed in different parts of the globe; this firſt operation, performed in a foreign country, might be looked upon as a kind of previous trial for the obſervers, a ſort of initiation, in which ſome difficulties, owing to the nature of the ſoil, but no true hardſhips, would be encountered. The whole of the work, except perhaps one ſingle ſtation, would be carried on in the King of Sardinia's dominions, and in ſome of the Republics of Switzerland; countries, where no moral nor political obſtacles would be ſtarted, and where the inhabitants, far from being troubleſome, would afford every aſſiſtance and

comfort in their power. I may add, that none of the mountains on which I have obſerved, and which are pointed out in the map as likely ſtations, preſent any danger.

If a ſurvey of that kind, executed in a mountainous country, is liable to ſome difficulties, it offers, on the other hand, advantages which perhaps more than overbalance thoſe difficulties.

Firſt, the viſual rays being leſs interrupted, the triangles become larger, and the ſtations fewer in number; whence the labour of the obſervers, and the chances of error, are by ſo much diminiſhed.

Secondly, Theſe ſame viſual rays proceeding through ſtrata of air leſs denſe and more free from the vapours which commonly thicken the lower parts of the atmoſphere, the danger of irregular refractions is by ſo much leſs, and the ſignals may be more diſtinctly perceived from great diſtances.

Beſides thoſe advantages which chiefly concern the meaſurement itſelf, the country would offer facilities for other natural enquiries, not unworthy the attention of philoſophical men, and which might eaſily be united with the capital object of theſe labours, with which object ſome of theſe enquiries are intimately united. I range among them,

The accurate determination of the length of the ſimple pendulum, which beats ſeconds in this mean latitude.

Experiments to be made on the oſcillations at different heights, with an invariable pendulum.

Experiments on the lateral attraction of mountains repeated and varied.

Obſervations on meteors, and ſeveral atmoſpherical phænomena relative to refractions, to heat, to hygrometry, to electricity, &c.

But,

But, above all, the improvement of barometrical meaſurements would moſtly deſerve the attention of the Commiſſioners. Nothing, as it ſeems, is now wanting in the theory of the operation; and it is only from a number of actual obſervations, made at different heights, and with every due precaution, compared with geometrical meaſurement, that the co-efficient (either conſtant or variable) to be applied as a correction for the atmoſpherical heat, will be obtained. That reſearch muſt be merely empirical; the effect ſought for being the reſult of many complicated cauſes, ſome of which are yet unknown. The real height of every ſtation being well determined, the time to be ſpent there for other purpoſes would allow a number of barometrical obſervations in varied circumſtances; and from theſe obſervations, rightly compared between themſelves, intereſting and uſeful reſults may juſtly be expected.

I have the honour to be, &c.

MARK AUGUSTE PICTET.

Geneva, Oct. 30, 1790.

Eclairciſſemens

Eclairciſſemens ſur la Carte, qui accompagne le Projet de Meſure préſenté à la Societé Royale, par M. A. Pictet.

(See Tab. II.)

DANS l'eſquiſſe de la ſuite de triangles à laquelle le préſent développement ſe rapporte, les montagnes ou les lignes tracées en rouge repréſentent des poſitions tirées des cartes, ou d'une inſpection générale de la direction des montagnes et des vallées *. Ce qui eſt deſſiné en noir indique les poſitions déterminées par des angles et des meſures réellement priſes; mais ſans prétendre cependant à plus de préciſion que la nature de la choſe ne l'exigeoit. Enfin ce qui eſt tracé legèrement en couleur noire montre ſeulement qu'il exiſte des montagnes là où elles ſont ainſi indiquées.

Les deux lignes rouges qui ſe coupent à angles droits dans la carte repréſentent le méridien et le parallèle de l'Obſervatoire de Genève dans l'étendue de la meſure propoſée; c'eſt-à-dire environ 1° 24′ pour le méridien, et environ 2°, pour le parallèle de longitude.

Le ſeul endroit dans lequel j'aye pû trouver un emplacement convenable pour une baſe rélative à la meſure de l'arc du méridien, eſt ſitué à l'oueſt de cet arc, et à-peu-près vis-à-vis du milieu de ſa longueur; ce ſont les marais de Choutagne. Ils offrent une plaine qui s'étend du nord au ſud dans une eſpace d'environ 2 lieues; elle eſt bornée à l'oueſt par le Rhône, et à l'eſt par une montagne très voiſine nommée la montagne de

* See Note, page 109.

Chou-

Choutagne, qui ſe prolonge au nord et au ſud beaucoup plus loin que la plaine elle même. Le ſol de cette plaine eſt ſi parfaitement horizontal qu'il paroit n'offrir d'autre élévation que celle qui réſulte de la courbure de la terre et d'une route qu'on a pratiquée au travers de cette même plaine de l'eſt à l'oueſt.

La ligne *a b* repréſente une baſe d'environ 25000 pieds Anglois de longueur, tracée dans cette plaine, et la ligne *b β* une autre baſe priſe dans la même plaine, et dont je parlerai tout à l'heure. Le ſol du marais eſt ferme le long de ces deux lignes, et en s'aidant de quelques ponts dans les endroits où ils ſeroient néceſſaires, la meſure ne ſouffriroit aucune difficulté. Une grande partie de la plaine eſt en prairie pendant l'été; il n'y a que très peu d'eau dans le reſte; je l'ai trouvé encore très praticable au milieu d'Octobre dernier, tems où j'y ai fait les opérations rélatives à la préſente eſquiſſe.

Les deux extrémités *a* et *b* de la baſe ſont marquées par deux bâtimens qui ſeroient utiles dans le voiſinage des deux ſtations: l'un eſt une petite ferme au point *a*, nommée *Chez Thevenet*, et l'autre une Briqueterie à l'extrémité *b*. Ces deux points ſont viſibles l'un de l'autre, et je remarque en paſſant que les inſtrumens pourroient arriver par eau depuis l'Angleterre juſques à la ſtation *b*, par la voye de Marſeille et du Rhône.

J'ajouterai que tout le côté oriental de la baſe, le long du pied de la montagne, eſt très peuplé; c'eſt un vignoble conſidérable, et l'on auroit tout-à-fait à-portée le logement et tous les ſecours néceſſaires au travail de la meſure; et quoique la plaine ſoit marécageuſe, le vignoble que la borde paſſe pour très ſain.

Il n'étoit pas facile de trouver ſur le ſommet de la montagne Choutagne deux ſtations qui réuniſſent les conditions néceſſaires; ſavoir 1^mo^, D'être viſibles des deux extrémités de la baſe qui

ſont

ſont abaiſſées de 8 à 10 degrés ſous l'horiſon. 2^do^, D'être viſibles l'une de l'autre. 3^tio^, D'être placées de manière qu'on put découvrir les ſommités à l'eſt du côte oppoſé à la baſe. 4^to^ enfin, D'être aſſez diſtantes entre elles pour fournir un côté convenable au premier triangle de la chaîne. Les points marqués *c* et *d* ſont les ſeuls, à ce que je crois, qui réuniſſent ces conditions. J'ai même quelque doute que la montagne de la Caille ſoit viſible du point *c* ſans élever l'inſtrument de quelques pieds. On voit que la diſtance *c d* de ces deux points eſt un peu moindre que la longueur de la baſe, et l'on trouvera ſans doute que l'angle que cette ligne ſouſtend ſur la montagne de la Caille eſt trop petit; il n'eſt effectivement que de 17 degrés, et ſous cette obliquité et à cette diſtance une erreur de 5″ en *c* ou en *d* allongeroit le côté oppoſé de pieds. Cette conſidération pourroit être jugée ſuffiſante pour engager les commiſſaires à préférer à la ſtation *c* une ſtation *e* que j'indique ſur le ſommet ſeptentrional du mont Colombier, quoique ce point ſoit ſitué ſur le territoire François. La baſe eſt placée très convenablement pour déterminer ce point, et ſi l'on abandonnoit ainſi la ſtation *c*, cette même baſe pourroit être prolongée du côté du nord d'environ 3 à 4000 pieds; en rapprochant un peu vers l'eſt ſon extrémité ſeptentrionale, en lui donnant la direction $b\beta$; elle ſeroit alors encore plus favorablement diſpoſée pour déterminer la ſtation *e*, d'où, juſques à la ſtation *d*, très bien déterminée par la même baſe ſur la montagne de Choutagne, la diſtance ſeroit d'environ 7500 toiſes, et l'angle oppoſé en *f* ſur la montagne de la Caille d'environ 34 degrés, c'eſt-à-dire double du précédent. Cette diſpoſition du premier triangle de la chaîne ſeroit ſans contredit plus avantageuſe que la précédente.

Le mont Colombier eſt élevé d'environ 650 toiſes * au-deſſus du niveau de la plaine; les points *c* et *d* ſur la montagne de Choutagne ſont élevés d'environ 380 toiſes, et la montagne de la Caille d'environ 200 toiſes ſeulement; c'eſt la plus baſſe de toutes celles qui ſervent à des ſtations; mais comme elle eſt tout-à-fait iſolée, elle eſt viſible de tous les côtés à une diſtance ſuffiſante.

Depuis les points *f* et *d* on détermine la ſtation *g* ſur la Tournette; et ces trois points forment le triangle central d'où l'on procède enſuite au nord et au ſud dans la direction de la méridienne. La Tournette eſt une montagne élevée d'environ 1000 toiſes au-deſſus du Lac de Genève; elle eſt iſolée, et offre un des points de vûe les plus frappans et les plus étendus qu'on puiſſe trouver dans ces régions. On apperçoit très bien depuis ce ſommet non ſeulement les deux extrémités de l'arc du méridien, mais encore preſque toutes les ſtations, excepté celles qui ſont dans le Vallais au-delà du glacier de Buët ou de la Mortine. Le ſommet de la montagne d'où j'ai obſervé mes angles n'eſt pas du plus facile accès; mais j'ái remarqué à environ 150 toiſes plus bas un endroit très acceſſible, nommé le Plan du Rivray, qui ſeroit convenable pour la ſtation. L'inſpection de la carte montre combien cette ſtation eſt importante, puiſqu'elle n'offre pas moins de ſept points à obſerver.

En partant du triangle central *dfg* pour aller au midi, on détermine par les ſtations *d* et *g* la ſtation *b* ſur la Dent de Nivolet. Cette montagne eſt très voiſine de Chambery, capitale

* Les hauteurs de la plupart des ſtations ne ſont indiquées qu'à-peu-près, et pour donner une idée des directions reſpectives des rayons viſuels rélativement à l'horiſon, et de la néceſſité d'avoir un inſtrument dont le mouvement dans les plans verticaux ſoit de la plus extrême préciſion.

de la Savoye; et quoique fort escarpée à l'ouest, elle est, m'a-t-on dit, d'un facile accès du côté de l'est.

Il existe vers le bord occidental du Lac du Bourget une montagne appellée la Dent du Chat, sur laquelle je suis monté, et dont j'ai déterminé la position au moyen d'une base qui est désignée sur la carte. Cette montagne est élevée d'environ 650 toises au-dessus du Lac du Bourget, et accessible, quoiqu'avec quelque difficulté; elle offriroit une station subsidiaire, dans le cas où celle sur la Dent de Nivolet seroit impraticable, ce qui n'est cependant nullement probable. La position de cette dernière seroit d'ailleurs infiniment meilleure rélativement à la suite de la chaîne.

Avec les stations *g* et *h* on détermine le point *i* sur une montagne que j'ai vûe depuis la Tournette, qui m'a paru très accessible, et qui termine la chaîne qui accompagne la rive droite de la rivière d'Arc depuis St. Jean de Maurienne. J'ignore le nom de cette montagne; et c'est ici que commence la partie de ma carte qui n'offre que des probabilités.

Des stations *b* et *i* on détermine le point *k*, où les cartes indiquent une montagne qu'elles nomment Coceiron. Cette montagne, formant l'extrémité d'une chaîne entre deux vallées dans la direction des deux stations *i* et *b*, doit être visible depuis l'une et l'autre. C'est aux environs de cet endroit que j'ai lieu de présumer que la méridienne de l'Observatoire de Genève, prolongée au sud depuis le sommet de la montagne de Salève, vient passer, sans être interceptée dans l'intervalle par aucune des montagnes qui s'y rencontrent.

Avec les stations *i* et *k* on détermine la station *l* sur la chaîne qui sépare la Maurienne de la Tarentaise, et dans laquelle j'ai vû plusieurs sommités accessibles: et enfin avec les stations

k et

k et *l* on détermine la ſtation *m* ſur le mont Sapey, où j'ai été, et d'où j'ai vû la direction générale des chaînes telle qu'elle eſt indiquée ſur la carte; et en particulier que le rayon viſuel *m k* eſt parfaitement libre. Du mont Sapey et du point *l*, choiſi convenablement, on détermine finalement St. Jean de Maurienne.

J'ai obſervé en montant le Sapey une très haute montagne inacceſſible, ſituée au midi de St. Jean de Maurienne, et dont le ſommet offre trois aiguilles contigues de la forme que j'ai deſſinée ſur la carte, et qui m'ont paru faciles à reconnoître de loin. Ces aiguilles, que j'ai très bien retrouvées enſuite depuis la Tournette, ſe nomment, à ce qu'on m'a dit, les Aiguilles d'Arve; et elles m'ont ſervi à déterminer à-peu-près la poſition de la ville de St. Jean rélativement à la méridienne de Genève; du moins à m'aſſurer qu'elle ne s'écarte pas beaucoup de cette méridienne. Je ne repéterai pas ici ce que j'ai dit dans le Mémoire ſur la convenance que je vois à la choiſir pour terme méridional de l'arc à meſurer.

Je reprends le triangle central *dfg* pour marcher de-là vers le nord de la chaîne des triangles.

Les ſtations *f* et *g* déterminent la ſtation *o* ſur le Pithon de Salève. C'eſt un rocher iſolé et du plus facile accès, qui domine la montagne; il eſt élevé d'environ 512 toiſes au-deſſus du Lac de Genève, et parfaitement viſible depuis l'Obſervatoire de cette ville.

Avec les ſtations *g* et *o* on obtient la ſtation *p* ſur le Môle. Cette montagne eſt élevée d'environ 750 toiſes au-deſſus du Lac de Genève; elle eſt de forme conique, parfaitement iſolée, d'un facile accès, et offre une des plus belles ſtations de toute la meſure; elle préſente d'autres avantages pour des expériences ſur

l'attraction latérale, pour des obſervations barométriques et météorologiques; en un mot, elle mérite toute l'attention des obſervateurs, auxquels elle offrira d'ailleurs une ſtation auſſi importante que celle de la Tournette.

Avec les ſtations *o* et *p* ſur le Pithon et ſur le Môle, on arrive à l'Obſervatoire de Genève, ſur lequel j'ai donné dans le Mémoire des détails qui me paroiſſent ſuffiſans; je le déſigne par la lettre *q*.

Avec les ſtations *o* et *q* on détermine un point quelconque *r*, acceſſible et habitable dans la chaîne du Mont Jura, aux environs du parallèle de l'Obſervatoire de Genève, et qui forme le terme occidental de la meſure; on a à cet égard beaucoup de choix; le glacier de Buet, ſur lequel ſe fera le ſignal, eſt viſible de tous les points du Jura près du parallèle de Genève. On trouve des habitations juſques vers le ſommet de cette derniere montagne, et on choiſira la plus convenable pour y établir l'obſervateur et ſa pendule.

La ligne *q p*, menée de l'Obſervatoire au Môle, ſert de baſe au triangle *qps*, dont le ſommet *s* eſt à Vincy, village du Pays de Vaud, viſible de l'Obſervatoire de Genève, et dans lequel j'ai une maiſon de campagne, que j'ai offerte dans mon Mémoire pour y faire les obſervations aſtronomiques dans le cas où l'on jugeroit à-propos d'étendre juſques là l'arc du méridien. Cet endroit eſt élevé d'environ 80 toiſes au-deſſus du Lac, et ſeroit commode pour les obſervateurs.

Ce triangle *qps* qui appartient à la méridienne, eſt auſſi le premier de la ſuite deſtinée à déterminer la longueur du parallèle à l'eſt. La ligne *p s*, menée du Môle à Vincy, et les ſtations *p* et *s*, déterminent l'une ou l'autre des Montagnes du Chablais appellées la Dent d'Oche et la Cornette. Elles ſont voiſines

l'une

l'une de l'autre, et je les déſigne toutes deux, parce que la Dent d'Oche ſur laquelle je ſuis monté eſt d'un aſſez difficile accès; la Cornette ſur laquelle je n'ai point été eſt, dit-on, très facile à monter, et a de plus l'avantage d'être un peu plus haute que la Dent d'Oche (qui eſt cependant élevée de 940 toiſes au deſſus du lac), et un peu plus à l'eſt du côté des montagnes du Vallais. Je préférerois donc la ſtation de la Cornette telle que je l'ai indiquée.

La ligne *tp* de la Cornette au Môle ſert de baſe au triangle *ptv*, dont le ſommet eſt à la Dent de Vaulion, montagne dans la chaîne de Jura, juſqu'où je propoſe d'étendre encore, ſi l'on veut, l'arc du méridien.

Cette même ligne *tp* ſert de baſe à un triangle *ptu*, dont le ſommet *u* eſt au glacier de Buët, qui ſera à la fois une ſtation pour meſurer des angles, et le lieu du ſignal pour les obſervations rélatives à la meſure aſtronomique de l'arc du parallèle. Cette montagne, élevée d'environ 1300 toiſes au-deſſus du Lac de Genève, eſt couverte d'une couche de neige permanente; mais cela n'empêcheroit pas qu'on ne pût y faire commodément les obſervations dans les mois d'Août ou de Septembre. On peut bâtir une petite cabane pour le ſéjour néceſſaire, ſur des rochers d'ardoiſe qui ſont voiſins de la ſommité. La montagne n'eſt pas d'un accès difficile par la route que M. Exchaquet a le premier indiquée, et que j'ai ſuivie à mon dernier voyage à cette montagne; et lors même qu'il reſteroit quelques difficultés à vaincre, cette ſtation eſt ſi belle et ſi importante, qu'on ne manqueroit pas de motifs et de courage pour les ſurmonter.

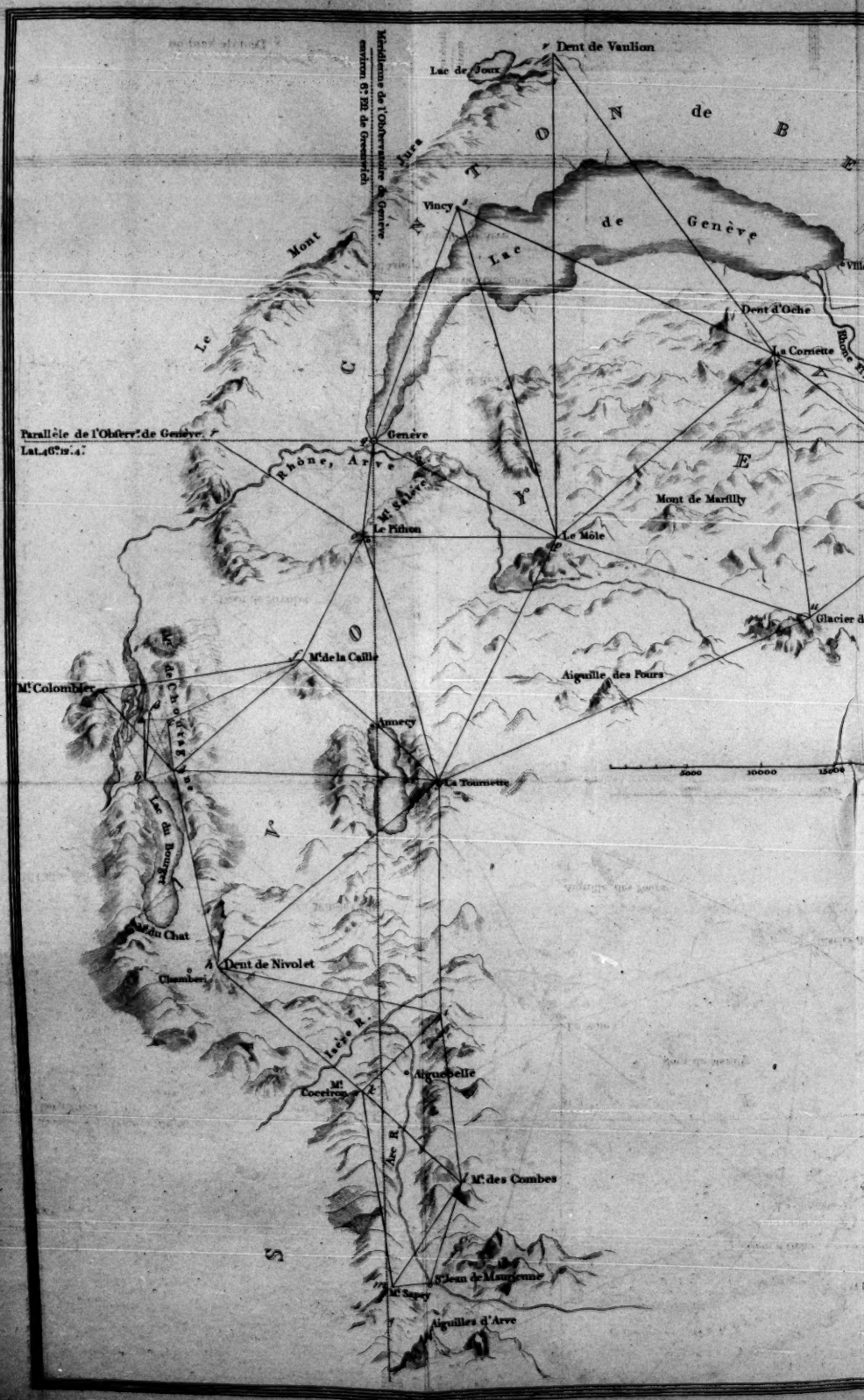
On remarquera qu'il ſe forme ainſi autour du Môle un exagone irrégulier, ce qui fournit l'occaſion d'une vérifica-

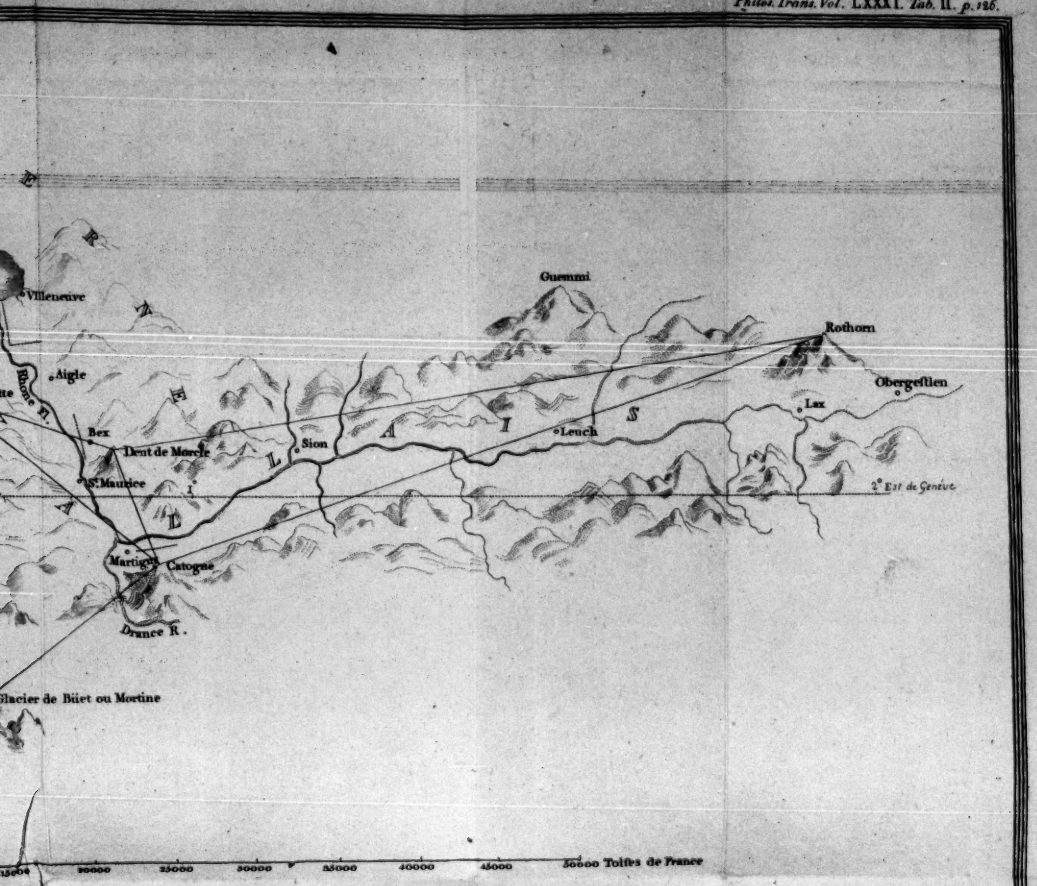
tion; et qu'entre autres, la ligne *pu* peut être déterminée de deux manieres; savoir, immédiatement par le triangle *pgu*, et en faisant le circuit que nous venons de faire par les stations *goust*.

Ici commence la partie de ma carte que je dois à la complaisance de mon ami M. WILD. Il me prévient que les positions qu'il m'a désignées ne sont pas exactes; mais il ne s'agissoit que d'établir la direction générale du haut et bas Vallais, de marquer les principales montagnes accessibles, et les endroits où l'on pourroit mesurer des bases de vérification. La carte du Vallais qu'il m'a envoyée, et dont la mienne offre l'extrait, m'a paru atteindre ce but. Il est évident que le triangle qui a pour base la distance de la Dent de Morcles à la Catogne, et qui se termine au Rothorn, non loin de la source du Rhône, auroit l'angle du sommet beaucoup trop aigu; mais il n'est pas douteux aussi que dans une vallée large et presque droite, comme l'est celle du haut Vallais, on peut former facilement une suite de triangles par des stations alternatives sur les sommets accessibles des deux côtés de la vallée, et arriver à l'extrémité par un petit nombre de ces triangles.

Les lignes droites, tracées en rouge dans cette partie de la carte, désignent les bases praticables; ces bases sont au nombre de quatre, mais aucune d'elles n'égale en longueur celle que j'ai trouvée dans les plaines de Choutagne.

Si le projet dont je viens de développer l'esquisse, est agréé par la Société Royale, mon premier soin, dès que la saison le permettra, sera de parcourir le haut Vallais pour découvrir le dernier sommet accessible du coté de l'est, d'où la Mortine ou le glacier de Buet soit visible. Ce point une fois trouvé,


Dent de Vaulion
Lac de Joux
Jura
Méridienne de l'Observatoire de Genève.
environ 6'. 22. de Greenwich
Vincy
Lac de Genève
Mont Le
Dent d'Oche
La Corniette
Parallèle de l'Observ.e de Genève
Lat. 46°12'.4.
Genève
Rhône, Arve
M.t Salève
Le Pithon
Le Môle
Mont de Marilly
M.t de la Caille
M.t Colombier
Aiguille des Pours
Glacier d
Amercy
La Tournette
Lac du Bourget
Mont du Chat
Dent de Nivolet
Chambery
Isère R.
Aiguebelle
M.t Cocetroz
Arc R.
M.t des Combes
S.t Jean de Maurienne
M.t Sapey
Aiguilles d'Arve
5000
10000
15000


Villeneuve
Aigle
Rhone R.
Bex
Dent de Morcle
St. Maurice
Martigny
Catogne
Drance R.
Sion
Leuch
Ouemmi
Rothorn
Obergestlen
Lax
2° Est de Genève
Glacier de Biiet ou Mortine
Toises de France

Rastre Sc.

j'établirai la ſuite de triangles pour y parvenir; et l'incertitude qui me reſte ſur l'étendue totale de l'arc du parallèle dont les deux extrémités ſont viſibles depuis la montagne dont je viens de parler, ſera levée.

METEOROLOGICAL JOURNAL

KEPT AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL.

for January 1790.

1790		Time.	Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.	Inch.	Points.	Str.	
Jan.	1	8 0	34	53	29,92		W	1	Fine.
		2 0	40	56,5	30,08		W	1	Fair.
	2	8 0	36	53,5	30,37		ENE	1	Cloudy.
		2 0	42	55	30,32		S by E	1	Cloudy.
	3	8 0	44	53	30,01	0,093	WSW	1	Cloudy.
		2 0	50	55,5	30,00		WNW	1	Cloudy.
	4	8 0	42	53,5	30,25				Foggy.
		2 0	45	55	30,26		NW	1	Cloudy.
	5	8 0	43	53	30,26				Foggy.
		2 0	45	55	30,24		ENE	1	Cloudy.
	6	8 0	40,5	52,5	30,20		E	1	Cloudy.
		2 0	40,5	53,5	30,22		SSE	1	Cloudy.
	7	8 0	40	51,5	30,42		WNW	1	Cloudy.
		2 0	41,5	54	30,47		WNW	1	Hazy.
	8	8 0	33,5	51	30,44		WNW	1	Foggy.
		2 0	35	53	30,40		WNW	1	Cloudy.
	9	8 0	32	50	30,30				Foggy.
		2 0	39	53	30,25		SSE	1	Fine.
	10	8 0	33,5	50	30,24		SW	1	Foggy.
		2 0	40	53	30,22		W	1	Cloudy.
	11	8 0	44	50,5	30,13		WSW	1	Cloudy.
		2 0	47	53,5	30,10		WSW	1	Cloudy.
	12	8 0	49,5	52,5	29,88	0,025	WSW	2	Cloudy.
		2 0	52,5	56,5	29,86		WSW	2	Cloudy.
	13	8 0	50,5	54	29,88		WSW	2	Cloudy.
		2 0	48,5	55	29,86		WSW	2	Rain.
	14	8 0	46	53	29,88	0,073	NW	1	Cloudy.
		2 0	47	56	29,90		NNE	1	Cloudy.
	15	8 0	41,5	53	29,94	0,030	N	1	Cloudy.
		2 0	40	56	29,99		W by N	1	Cloudy.
	16	8 0	40	53	30,01	0,137	WNW	2	Cloudy.
		2 0	45	55	30,11		WNW	2	Cloudy.

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1790	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
		°	°	Inches.	Inch.	Points.	Str.	
Jan. 17	8 o	38,5	53	30,39		NW	1	Cloudy.
	2 o	42	55	30,39		NNE	1	Cloudy.
18	8 o	38	52	30,22		ENE	2	Cloudy.
	2 o	40	53,5	30,12		ENE	2	Fine.
19	8 o	33	50	30,14		E	2	Cloudy.
	2 o	38	52,5	30,16		E	2	Fine.
20	8 o	32	49,5	30,25		E	2	Fine.
	2 o	35	51,5	30,31		E	2	Fine.
21	8 o	32	50	30,44		E	1	Fair.
	2 o	39	53	30,41		E	1	Fine.
22	8 o	33	50,5	30,38		E	1	Foggy.
	2 o	38	52,5	30,37		WNW	1	Cloudy.
23	8 o	42	52	30,28		WNW	1	Cloudy.
	2 o	47	54	30,28		WNW	1	Cloudy.
24	8 o	43	52	30,20		WNW	1	Cloudy.
	2 o	47	54,5	30,17		WNW	1	Cloudy.
25	8 o	42	52,5	30,11	0,250	NNE	1	Rain.
	2 o	43,5	55	30,20		N	1	Fair.
26	8 o	35,5	50,5	30,22		WNW	1	Cloudy.
	2 o	41	52,5	30,11		W	1	Fair.
27	8 o	39,5	51,5	29,35	0,181	WNW	2	Fair.
	2 o	42	55	29,35		WNW	2	Fair.
28	8 o	39	52	29 56		W	1	Cloudy.
	2 o	48	54,5	29,27		SW	2	Cloudy.
29	8 o	37	52,5	29,28	0,160	WNW	2	Fine.
	2 o	41,5	54,5	29,28		WNW	2	Fine.
30	8 o	38	52,5	29,51		NW	2	Cloudy.
	2 o	42,5	54,5	29,69		NW	1	Fine.
31	8 o	38,5	52,5	29,62	0,018	WNW	1	Cloudy.
	2 o	44	54,5	29,63		WNW	1	Fine.

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1790	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Feb. 1	8	0	37	52	29,88		WNW	1	Fine.
	2	0	43	56	30,02		NW	1	Fine.
2	8	0	37	52	30,20		WNW	1	Fair.
	2	0	45,5	54	30,24		W	1	Fair.
3	8	0	43	52,5	30,38		W	1	Cloudy.
	2	0	47	55	30,43		W	1	Cloudy.
4	8	0	44	53	30,61		W	1	Cloudy.
	2	0	46	54	30,62		W	1	Cloudy.
5	8	0	43,5	52	30,61		W	1	Cloudy.
	2	0	46,5	54,5	30,58		W	1	Cloudy.
6	8	0	42	53	30,51		WNW	1	Fair.
	2	0	48,5	56	30,51		WNW	1	Fine.
7	8	0	42	53	30,45		WNW	1	Cloudy.
	2	0	42,5	56	30,37		WNW	1	Cloudy.
8	8	0	38	52,5	30,25		NW	1	Cloudy.
	2	0	39,5	53,5	30,22		WNW	1	Cloudy.
9	8	0	40	52,5	30,08		WNW	1	Cloudy.
	2	0	43	53	30,03		WNW	1	Cloudy.
10	8	0	42,5	53	30,07		NE	1	Cloudy.
	2	0	43	54	30,25		E	1	Cloudy.
11	8	0	39	52,5	30,22		WSW	1	Cloudy.
	2	0	49	55	30,22		W	1	Cloudy.
12	8	0	44	53	30,13		W	1	Cloudy.
	2	0	50	57	30,17		W	1	Fair.
13	8	0	40,5	54	30,42		W	1	Fine.
	2	0	49	58	30,34		W	1	Cloudy.
14	8	0	42	54	30,18		WSW	1	Cloudy.
	2	0	48	56	30,10		WSW	1	Cloudy.
15	8	0	36	53	30,24		W	1	Fine.
	2	0	45	56	30,23		W	1	Fine.
16	8	0	43,5	53,5	30,00		S by W	2	Cloudy.
	2	0	44	56	29,95		WSW	2	Cloudy.

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1790	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Feb. 17.	8	0	35	54	30,22		WNW	1	Fine.
	2	0	40	56,5	30,26		WNW	1	Fair.
18	8	0	38	54	30,43		WNW	1	Foggy.
	2	0	46	57	30,45		WNW	1	Fine.
19	8	0	40	54	30,49		SW	1	Cloudy.
	2	0	47	58	30,46		SW	1	Fine.
20	8	0	35,5	53,5	30,44		N	1	Foggy.
	2	0	44	57	30,43		N	1	Hazy.
21	8	0	33	53	30,40				Foggy.
	2	0	40,5	57	30,38		NE	1	Hazy.
22	8	0	37	53	30,22		NE	1	Fine.
	2	0	49	57,5	30,15		S by W	1	Fine.
23	8	0	45,5	55	30,00	0,115	E	1	Cloudy.
	2	0	50,5	57	30,00		WNW	1	Cloudy.
24	8	0	44	55	30,17		WNW	1	Cloudy.
	2	0	50	57	30,17		SW	1	Cloudy.
25	8	0	49	55,5	30,04		SW	1	Cloudy.
	2	0	55	58	30,11		WSW	1	Cloudy.
26	8	0	49	56	30,02		SW	2	Cloudy.
	2	0	53	58,5	29,88		SW	2	Cloudy.
27	8	0	40,5	56	30,23		WNW	2	Fine.
	2	0	50	59,5	30,29		WNW	2	Fair.
28	8	0	42,5	56	30,25		W	1	Cloudy.
	2	0	50	59,5	30,25		WNW	1	Fair.

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1790	Time. H.	Time. M.	Therm. without	Therm. within.	Barom.	Rain.	Winds. Points.	Winds. Str.	Weather.
			o	o	Inches.	Inch.			
Mar. 1	7	0	46	56	30,35		NW	1	Cloudy.
	2	0	50	58	30,38		NW	1	Cloudy.
2	7	0	46,5	56	30,32		W	1	Cloudy.
	2	0	53	58	30,38		NNW	1	Cloudy.
3	7	0	47	56	30,38		NW	1	Cloudy.
	2	0	54	59	30,39		NW	1	Fair.
4	7	0	40	55,5	30,28		NW	1	Fine.
	2	0	49	59	30,27		NW	1	Fine.
5	7	0	40	56	30,32		NNW	1	Cloudy.
	2	0	46	59	30,39		NNW	1	Fair.
6	7	0	36	55	30,46		NE	1	Fair.
	2	0	44	57	30,45		NW	1	Fair.
7	7	0	40	54	30,44		NE	1	Cloudy.
	2	0	46	57	30,44		N	1	Cloudy.
8	7	0	41	54	30,44		ENE	1	Cloudy.
	2	0	47	55,5	30,42		S	1	Cloudy.
9	7	0	39	53	30,33		W	1	Cloudy.
	2	0	50,5	55,5	30,18		W	2	Cloudy.
10	7	0	43	33,5	29,88		W	2	Fair.
	2	0	51	56	29,91		W	2	Fair.
11	7	0	37	53,5	30,24		W	2	Fine.
	2	0	51	56	30,30		W	2	Cloudy.
12	7	0	48	54	30,30	0,042	W	2	Cloudy.
	2	0	56	58	30,32		W	2	Fair.
13	7	0	50,5	56,5	30,35		W	2	Cloudy.
	2	0	56	58,5	30,38		W	2	Cloudy.
14	7	0	38	55	30,55	0,080	W	1	Fine.
	2	0	50	56	30,52		NW	1	Fine.
15	7	0	35	54	30,59		NNW	1	Fine.
	2	0	46	55,5	30,62		N	1	Cloudy.
16	7	0	35	53,5	30,65		NNE	1	Fair.
	2	0	46	57	30,62		NNE	1	Fair.

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for March 1790.

1790	Time. H.	M	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Mar. 17	7	0	33	52,5	30,63		NE	1	Fair.
	2	0	43,5	56,5	30,63		E	1	Fine.
18	7	0	38	53	30 60		NE	1	Cloudy.
	2	0	47	57	30,56		NE	1	Fair.
19	7	0	42	54	30,45		NNE	1	Cloudy.
	2	0	49	57,5	30,44		NE	1	Fine.
20	7	0	38	54,5	30,42		E	2	Fine.
	2	0	48	57	30,42		E	1	Fine.
21	7	0	34,5	54	30,40		NE	2	Fine.
	2	0	49	59	30,33		E	2	Fine.
22	7	0	38,5	53,5	30,17		ENE	1	Fine.
	2	0	51,5	58,5	30,06		E	2	Fine.
23	7	0	40,5	54	29,85		E	1	Cloudy.
	2	0	50	57	29,85		ENE	1	Cloudy.
24	7	0	47,5	54	29,83		E	1	Cloudy.
	2	0	52	59	29,84		SW	1	Cloudy.
25	7	0	42	55	30,02		WSW	1	Cloudy.
	2	0	54	59	30,06		SW	1	Fair.
26	7	0	43	56	30,06		E	1	Cloudy.
	2	0	53	59	30,05		E	1	Fair.
27	7	0	43	55,5	30,01		E	1	Cloudy.
	2	0	53	59	30,00		E	1	Fair.
28	7	0	41	50,5	30,00		ENE	1	Cloudy.
	2	0	53	58	30,00		ENE	1	Fair.
29	7	0	42	56	30,00		ENE	1	Cloudy.
	2	0	48	58	30,00		NE	1	Cloudy.
30	7	0	42	56	30,00		NE	1	Cloudy.
	2	0	48	58	29,99		ENE	1	Cloudy.
31	7	0	41	55	29,96		NE	1	Fair.
	2	0	52	60	29,96		E	1	Fine.

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for April 1790.

1790	Time. H.	M.	Therm without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
April 1	7	0	42	55	30,07		E	2	Fair.
	2	0	42,5	57	30,08		E	2	Fair.
2	7	0	37	52	30,25		E	2	Fine.
	2	0	43	57	30,26		E	2	Fine.
3	7	0	35	53,5	30,30		N	2	Fine.
	2	0	48	58	30,25		NE	2	Fine.
4	7	0	39	53,5	30,23		NE	2	Fine.
	2	0	49	58,5	30,22		E	2	Fine.
5	7	0	37	54	30,26		NNE	2	Fair.
	2	0	47,5	58	30,22		E	2	Cloudy.
6	7	0	40	53,5	30,08		E	2	Cloudy.
	2	0	42	59	30,00		E	2	Fair.
7	7	0	38	53	29,80		NNE	2	Fair.
	2	0	47	59	29,74		E	2	Fine.
8	7	0	43	55	29,69		E	2	Fair.
	2	0	50	58,5	29,71		E	2	Fine.
9	7	0	40	55	29,62		E	2	Cloudy.
	2	0	47	57	29,55		E	2	Cloudy.
10	7	0	36	53,5	29,46	0,173	ENE	2	Rain.
	2	0	39,5	56	29,48		NE	2	Cloudy.
11	7	0	37	53	29,38	0,024	NE	2	Cloudy.
	2	0	38,5	55	29,44		NNE	2	Snow.
12	7	0	37	53	29,40	0,054	N	2	Cloudy.
	2	0	43	54,5	29,48		N	2	Cloudy.
13	7	0	38	52,5	29,61		N	2	Cloudy.
	2	0	40,5	55	29,66		NNE	2	Rain.
14	7	0	37	52	29,82	0,140	NW	1	Cloudy.
	2	0	42,5	56	29,86		NW	2	Cloudy.
15	7	0	36	53	29,76		ENE	1	Rain.
	2	0	42,5	55,5	29,45		ENE	1	Rain.
16	7	0	38	52	29,72	0,593	N	1	Cloudy.
	2	0	43	55	29,87		N	1	Cloudy.

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1790	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str	Weather.
April 17	7	0	35	52	29,98		NNW	1	Fine.
	2	0	40	56	29,96		N	1	Fair.
18	7	0	35	52	30,01		N	1	Fine.
	2	0	46	56	30,02		E	2	Cloudy.
19	7	0	35,5	52,5	30,05		N	2	Fine.
	2	0	45	55,5	30,05		N	2	Cloudy.
20	7	0	39	52,5	30,29		E	1	Cloudy.
	2	0	46	56	30,29		E	1	Fine.
21	7	0	39	52	30,24		E	1	Fine.
	2	0	50	55	30,20		S	1	Cloudy.
22	7	0	46	51,5	29,96		E by S	1	Cloudy.
	2	0	55	55,5	29,84		S	1	Fair.
23	7	0	51	54,5	29,72		WSW	1	Fair.
	2	0	58	59	29,66		WSW	2	Cloudy.
24	7	0	48	56	29,49	0,200	SW by W	1	Cloudy.
	2	0	53	59	29,50		WSW	1	Cloudy.
25	7	0	45	56	29,66	9,035	WSW	1	Cloudy.
	2	0	54	59	29,63		WSW	1	Cloudy.
26	7	0	44	56	29,73	0,025	WSW	1	Cloudy.
	2	0	54	59,5	29,80		SW	1	Rain.
27	7	0	42	57	29,98	0,201	N	1	Cloudy.
	2	0	55	59,5	30,04		N	1	Cloudy.
28	7	0	41	56	30,10		NW	1	Fine.
	2	0	55	59,5	30,02		SW	1	Cloudy.
29	7	0	48,5	57	29,83		SSW	2	Cloudy.
	2	0	54	58,5	29,73		SSW	2	Cloudy.
30	7	0	47	57	29,52	0,025	SW	2	Fair.
	2	0	56	59,5	29,62		SW	2	Fair.

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1790	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 1	7	0	48	58	29,63		S Eby E	1	Cloudy.
	2	0	51	60,5	29,55		E	1	Rain.
2	7	0	48	58,5	29,50	0,175	SSW	2	Cloudy.
	2	0	57,5	61	29,61		SSW	2	Fair.
3	7	0	47	58	29,86		WSW	1	Fair.
	2	0	59,5	61	29,87		NW	1	Fair.
4	7	0	47	58	30,04		W	1	Fine.
	2	0	62	61,5	30,04		SW	1	Fine.
5	7	0	52,5	58,5	29,92		W	1	Cloudy.
	2	0	55	62	29,88		NNW	1	Cloudy.
6	7	0	48	58,5	29,75	0,110	W	1	Rain.
	2	0	51	62	29,72		N	1	Rain.
7	7	0	49	60	29,68	0,280	WNW	1	Cloudy.
	2	0	57	63	29,74		E	1	Cloudy.
8	7	0	51	60,5	29,83		N	1	Cloudy.
	2	0	60,5	63,5	29,83		NNE	1	Fair.
9	7	0	52	61	29,94		N	1	Fine.
	2	0	59,5	64	29,98		N	1	Fair.
10	7	0	51,5	62	30,01	0,086	N	1	Cloudy.
	2	0	58	62	30,02		E	1	Cloudy.
11	7	0	51	62	30,06		N	1	Cloudy.
	2	0	64	64	30,06		N	1	Fair.
12	7	0	52	61,5	30,09		NE	1	Fine.
	2	0	62	66	30,09		N	2	Fine.
13	7	0	50	61	30,14		NE	2	Fine.
	2	0	63,5	64,5	30,11		NE	2	Fine.
14	7	0	50	61	30,10		NE	1	Fine.
	2	0	64	66	30,09		N	1	Fine.
15	7	0	50,5	61,5	30,03		NW	1	Hazy.
	2	0	61,5	65	29,99		WNW	1	Fine.
16	7	0	54	62	29,91		ESE	1	Fine.
	2	0	66	64	29,86		E	2	Fine.

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1790	Time. H.	M.	Therm. without °	Therm. within. °	Barom, Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 17	7	0	56	62,5	29,68		SW	2	Fine.
	2	0	57	64,5	29,66		WSW	2	Fair.
18	7	0	51	57	29,88	0,130	W	1	Fine.
	2	0	60	60	29,88		W	2	Cloudy.
19	7	0	53	59	29,76		S	2	Fine.
	2	0	63	60	29,69		S	2	Cloudy.
20	7	0	50	59	29,77	0,131	WSW	2	Fine.
	2	0	59,5	60	29,77		WSW	2	Cloudy.
21	7	0	50	58,5	29,96		W	1	Fine.
	2	0	59,5	59,5	29,96		W	1	Cloudy.
22	7	0	50,5	58,5	29,94	0,033	S	1	Cloudy.
	2	0	57	59	29,86		SE	1	Cloudy.
23	7	0	56	58,5	29,80	0,190	WSW	1	Cloudy.
	2	0	64	60	29,84		S	1	Cloudy.
24	7	0	54	60	29,80	0,201	E	1	Rain.
	2	0	63	61	29,72		S by W	2	Cloudy.
25	7	0	53	60,5	29,85	0,200	S	1	Rain.
	2	0	61	60,5	29,85		NE	1	Cloudy.
26	7	0	54	61	29,94	0,011	E	1	Cloudy.
	2	0	64	61	29,98		E	1	Cloudy.
27	7	0	55	61	30,00	0,223	NW	1	Cloudy.
	2	0	64	62,5	29,96		WNW	1	Fair.
28	7	0	58	62	30,00	0,881	NE	1	Cloudy.
	2	0	66	63,5	29,98		ENE	1	Fair.
29	7	0	58	62,5	29,99	0,137	E	1	Cloudy.
	2	0	63	63	30,02		S	1	Cloudy.
30	7	0	58	63	30,05		W	1	Cloudy.
	2	0	63	63	30,04		W	1	Cloudy.
31	7	0	56	62	29,97	0,110	WNW	1	Cloudy.
	2	0	63	63	30,02		N	1	Cloudy.

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for June 1790.

1790	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inch.	Points.	Str.	
June 1	7	0	56	62	30,17	0,022	W	1	Fine.
	2	0	66	62,5	30,12		SW	1	Fair.
2	7	0	60	62,5	30,16		N	1	Fair.
	2	0	66,5	64,5	30,16		N	1	Fair.
3	7	0	59	63	30,16		W	1	Cloudy.
	2	0	64	63	30,07		W	1	Cloudy.
4	7	0	59,5	63	30,07		W	1	Fair.
	2	0	65	64,5	30,08		NW	1	Fair.
5	7	0	55	62	30,20		WNW	1	Fair.
	2	0	63	63,5	30,16		WNW	1	Fair.
6	7	0	56,5	61,5	30,17		W	1	Fine.
	2	0	63	62	30,16		WNW	1	Fair.
7	7	0	57	61	29,96	0,040	WNW	1	Cloudy.
	2	0	65	63	29,96		WNW	1	Fair.
8	7	0	56	61	29,92		WSW	2	Fine.
	2	0	63	61,5	29,79		SSW	2	Cloudy.
9	7	0	58	61	29,56	0,060	SSW	2	Fair.
	2	0	63	61,5	29,49		SSW	2	Fair.
10	7	0	57	60	29,58		W	2	Fair.
	2	0	60	61	29,66		S	1	Rain.
11	7	0	53	60,5	29,89	0,340	NNW	1	Fair.
	2	0	64	62	29,85		WNW	1	Fair.
12	7	0	55	60,5	29,98		NW	1	Cloudy.
	2	0	62	62	30,02		NNW	1	Fair.
13	7	0	53	60	30,26		N	1	Fine.
	2	0	61	62	30,27		N	1	Fair.
14	7	0	56	60	30,33				Fine.
	2	0	66	62	30,30				Fine.
15	7	0	61	61,5	30,29		N	1	Fair.
	2	0	67	63,5	30,28		E	1	Cloudy.
16	7	0	54	61,5	30,24		NE	1	Cloudy.
	2	0	63	62	30,19		NE	1	Cloudy.

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1790	Time. H.	Time. M.	Therm. without	Therm. within.	Barom.	Rain.	Winds. Points.	Winds. Str.	Weather.
			°	°	Inches.	Inch.			
June 17	7	0	58	61	30,08		NE	1	Cloudy.
	2	0	65	62,5	30,01		E	1	Cloudy.
18	7	0	58	61	29,88	0,051	W	1	Fair.
	2	0	68	63	29,94		W	2	Cloudy.
19	7	0	59,5	62	30,00		SW	2	Cloudy.
	2	0	69	64	30,01		SW	2	Cloudy.
20	7	0	60	62,5	30,23		WSW	1	Fair.
	2	0	69,5	65	30,28		WSW	2	Fine.
21	7	0	65	65	30,35		WSW	1	Fine.
	2	0	74	67,5	30,35		WSW	1	Fine.
22	7	0	69	67	30,20				Fine.
	2	0	86	73,5	30,10		SSW	1	Fine.
23	7	0	71	71	29,96		W	1	Cloudy.
	2	0	76	72	29,96		W	1	Cloudy.
24	7	0	60	66	29,98		W	2	Fair.
	2	0	68	68	29,91		W	2	Cloudy.
25	7	0	59	65	29,87		WSW	2	Fair.
	2	0	68	67	29,87		WSW	2	Fair.
26	7	0	57	65	29,89		W	2	Cloudy.
	2	0	66,5	66,5	29,89		WNW	2	Fair.
27	7	0	53	64,5	29,96	0,120	NW	1	Fair.
	2	0	64	66	30,02		NW	1	Cloudy.
28	7	0	53	63	30,08	0,075	WNW	1	Cloudy.
	2	0	64	65	30,07		W	1	Fair.
29	7	0	55	63	30,02		SW	1	Cloudy.
	2	0	69,5	63,5	29,97		SSE	1	Cloudy.
30	7	0	53	63	29,93		SW	1	Cloudy.
	2	0	68	64	29,91		SW	2	Cloudy.

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for July 1790.

1790	Time. H.	M	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
July 1	7	0	60	63,5	29,82	0,048	WSW	2	Cloudy.
	2	0	68	64	29,83		WSW	2	Fair.
2	7	0	56,5	63	29,72		WSW	1	Fair.
	2	0	63	63,5	29,63		SW	1	Rain.
3	7	0	54,5	63	29,68	0,088	W	1	Fine.
	2	0	70	64	29,68		WNW	1	Cloudy.
4	7	0	55	63	29,70		SW	1	Fair.
	2	0	66	64	29,63		SSW	2	Cloudy.
5	7	0	57	62,5	29,32	0,087	WSW	1	Cloudy.
	2	0	67,5	63,5	29,29		W	1	Cloudy.
6	7	0	55	62,5	29,64	0,155	NW	2	Cloudy.
	2	0	64,5	63,5	29,80		NNW	2	Cloudy.
7	7	0	55	62	30,10	0,014	N	2	Fine.
	2	0	67	63	30,16		NW	1	Fair.
8	7	0	55	62	30,20		SW	1	Fair.
	2	0	70	63,5	30,14		SW	1	Cloudy.
9	7	0	58	63	29,87	0,320	SW	2	Cloudy.
	2	0	67	64	29,78		SW	2	Fair.
10	7	0	55	63	29,87	0,090	SW	1	Cloudy.
	2	0	65	63	29,78		WSW	1	Cloudy.
11	7	0	55,5	62	29,55		W	1	Cloudy.
	2	0	64	63	29,55		NNW	1	Cloudy.
12	7	0	53	61,5	29,68	0,069	W	1	Cloudy.
	2	0	62	62,5	29,67		W	1	Cloudy.
13	7	0	56	61,5	29,60	0,063	W	1	Cloudy.
	2	0	65	62,5	29,46		SSW	2	Cloudy.
14	7	0	55	61,5	29,51	0,205	WSW	1	Cloudy.
	2	0	61	62	29,55		W	1	Cloudy.
15	7	0	55	61	29,75	0,050	W	1	Fine.
	2	0	68	61,5	29,84		W	1	Cloudy.
16	7	0	58,5	61	29,95		W	1	Fair.
	2	0	69	62,5	30,03		NW	1	Fair.

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for July 1790.

1790	Time. H.	Time. M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
July 17	7	0	61	62	30,16		NW	1	Cloudy.
	2	0	70	63,5	30,16		NW	1	Cloudy.
18	7	0	61	63	30,08		W	1	Cloudy.
	2	0	71	64	30,04		SW	1	Cloudy.
19	7	0	59	64	30,06	0,060	W	1	Fine.
	2	0	72	65	30,04		W	1	Fine.
20	7	0	57	63,5	29,80	0,015	SW	2	Cloudy.
	2	0	64	64,5	29,63		SW	2	Cloudy.
21	7	0	57	63,5	29,77	0,026	W	2	Fair.
	2	0	65	64	29,86		WNW	2	Cloudy.
22	7	0	55	63	30,06		W	1	Cloudy.
	2	0	70,5	64	30,06		WNW	1	Fair.
23	7	0	59	63	30,00	0,015	WNW	1	Cloudy.
	2	0	59	63,5	29,99		W	1	Rain.
24	7	0	62	63,5	30,00	0,060	WNW	1	Cloudy.
	2	0	71	65	30,03		WNW	1	Cloudy.
25	7	0	58	64	30,13		WNW	1	Cloudy.
	2	0	71,5	65,5	30,14		W	1	Fine.
26	7	0	59	64	30,16		WSW	1	Cloudy.
	2	0	73,5	65,5	30,14		W	1	Fair.
27	7	0	59	64,5	30,15		W	1	Cloudy.
	2	0	73,5	66,5	30,15		WNW	1	Fair.
28	7	0	59,5	65	30,10		E	1	Cloudy.
	2	0	73	67	29,96		SSE	1	Fine.
29	7	0	58	65,5	29,60		NW	1	Cloudy.
	2	0	67	65,5	29,58		NW	1	Cloudy.
30	7	0	55	64	29,60	0,030	W	1	Cloudy.
	2	0	66,5	64	29,64		W	1	Cloudy.
31	7	0	56	63,5	29,46	0,303	WSW	2	Cloudy.
	2	0	65	64,5	29,61		WSW	2	Cloudy.

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for Auguſt 1790.

1790	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Aug. 1	7	0	53	63	29,93	0,012	W	1	Fine.
	2	0	66	64,5	30,00		W	1	Fair.
2	7	0	56	62,5	29,91		SW	1	Cloudy.
	2	0	59	62,5	29,81		SSW	1	Rain.
3	7	0	55	62	29,74	0,202	WSW	1	Cloudy.
	2	0	65	62,5	29,66		W	1	Rain.
4	7	0	54	61,5	29,83	0,081	W	1	Cloudy.
	2	0	67,5	62,5	29,86		W	1	Fair.
5	7	0	55	61,5	30,04		W	1	Cloudy.
	2	0	69	62	30,00		W	1	Cloudy.
6	7	0	60	62	29,91		W	1	Cloudy.
	2	0	66,5	63,5	29,86		WSW	1	Rain.
7	7	0	64	63,5	30,01	0,025	WSW	1	Cloudy.
	2	0	76	65	30,01		W	1	Cloudy.
8	7	0	62	65	29,88		W	1	Cloudy.
	2	0	71	66	29,94		W	1	Cloudy.
9	7	0	57,5	64,5	30,00		WNW	1	Fine.
	2	0	70	66	30,00		NW	1	Fine.
10	7	0	56,5	64,5	30,07		W	1	Fine.
	2	0	75,5	67	30,06		W	1	Fine.
11	7	0	50,5	65	30,14		W	1	Hazy.
	2	0	75	67	30,11		W	1	Hazy.
12	7	0	58	66	30,11		WSW	1	Fair.
	2	0	73	67	30,10		SW	1	Cloudy.
13	7	0	60,5	67	30,04		W	1	Fair.
	2	0	74	68	30,02		W	1	Cloudy.
14	7	0	60	67	30 10		WNW	1	Fine.
	2	0	72,5	68,5	30,10		NW	1	Fine.
15	7	0	59	67	30,02		SSE	1	Fine.
	2	0	75,5	69,5	29,95		SSW	1	Fine.
16	7	0	66	68	29,90		WSW	1	Cloudy.
	2	0	73	69	29,93		WSW	1	Cloudy.

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for August 1790.

1790	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Aug. 17	7	0	60	68	29,95	0,020	W	1	Fair.
	2	0	68	68,5	30,01		WNW	1	Rain.
18	7	0	60	68	30,16	0,061	WNW	1	Cloudy.
	2	0	71	68	30,13		WNW	1	Fair.
19	7	0	59	67	30,02		W	1	Cloudy.
	2	0	72,5	68	30,00		W	1	Cloudy.
20	7	0	63	67,5	30,05		W	1	Cloudy.
	2	0	77	69,5	30,05		WSW	1	Fine.
21	7	0	65	68	29,78	0,060	SSE	1	Fine.
	2	0	76	73	29,73		W	1	Hazy.
22	7	0	56	68	30,04		W	1	Fine.
	2	0	67	68	30,08		W	1	Fine.
23	7	0	62	67	29,94		WSW	2	Cloudy.
	2	0	67	67,5	29,94		WSW	2	Cloudy.
24	7	0	53	65,5	30,01		W	1	Fine.
	2	0	68	66,5	29,96		W	2	Fair.
25	7	0	53	64	29,93		W	1	Cloudy.
	2	0	59,5	64,5	29,85		SE	1	Rain.
26	7	0	57	64,5	29,64	1,150			Rain.
	2	0	63	65	29,73		NW	1	Cloudy.
27	7	0	55	64	29,83	0,035	NW	1	Cloudy.
	2	0	58,5	64	29,80		N	1	Cloudy.
28	7	0	51,5	62	29,95	0,345	WNW	1	Cloudy.
	2	0	63	63,5	29,95		W	1	Cloudy.
29	7	0	53,5	62	30,02		W	1	Cloudy.
	2	0	59	62	30,04		W	1	Cloudy.
30	7	0	54	61,5	30,15		W	1	Cloudy.
	2	0	67	62,5	30,15		WSW	1	Fair.
31	7	0	59	62	30,08		WSW	2	Cloudy.
	2	0	73	64	30,06		W	2	Cloudy.

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for September 1790.

1790	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Sept. 1	7	0	62	63,5	29,91		W	1	Cloudy.
	2	0	68	64	29,88		W	1	Cloudy.
2	7	0	52,5	62,5	29,82		W	1	Fair.
	2	0	64	62,5	29,60		W	1	Cloudy.
3	7	0	49,5	61	29,31	0,210	W	2	Fine.
	2	0	58	61,5	29,43		W	2	Cloudy.
4	7	0	48	60	29,64		WNW	1	Fine.
	2	0	61	62	29,64		W	1	Cloudy.
5	7	0	45	59,5	29,67		W	1	Fine.
	2	0	55	61	29,77		WNW	1	Fair.
6	7	0	46	59	30,00		N	1	Fine.
	2	0	60	60	30,03		NNW	1	Fair.
7	7	0	46	58,5	30,11		NNW	1	Fine.
	2	0	55,5	60	30,05		N	1	Rain.
8	7	0	49	58,5	30,06	0,022	N by W	1	Fine.
	2	0	58	61,5	30,12		NNW	1	Fair.
9	7	0	49	58,5	30,19		W	1	Cloudy.
	2	0	62,5	59,5	30,12		W	1	Fair.
10	7	0	57	59	29,96	0,053	W	1	Cloudy.
	2	0	64	60	29,98		W	1	Fair.
11	7	0	54	59,5	29,98		W	1	Fine.
	2	0	64	61	30,04		W	1	Fair.
12	7	0	55,5	59,5	30,11		W	1	Cloudy.
	2	0	63,5	60,5	30,11		W	1	Cloudy.
13	7	0	56,5	60,5	30,00		W	1	Cloudy.
	2	0	67	62	29,94		W	1	Fair.
14	7	0	56	61,5	29,80		SSW	2	Cloudy.
	2	0	65	62,5	29,75		SW	2	Fair.
15	7	0	57	62	29,78		SSW	1	Cloudy.
	2	0	67	63,5	29,75		WSW	1	Fair.
16	7	0	49	61,5	29,98		WSW	1	Fair.
	2	0	62	62,5	30,10		W	1	Fine

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for September 1790.

1790	Time. H.	Time. M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Sept. 17	7	0	48	61	30,33		W	1	Fair.
	2	0	63	63	30,26		SW	1	Fine.
18	7	0	46,5	61	30,15		SW	1	Fine.
	2	0	68	64	30.08		SW	1	Fine.
19	7	0	56	62,5	29,85		SW	1	Cloudy.
	2	0	68	65	29.72		SW	2	Fair.
20	7	0	52	63,5	29,64	0,020	WSW	2	Fine.
	2	0	62	64	29,68		W	2	Fine.
21	7	0	50	60,5	29,83		W	2	Fine.
	2	0	59	61,5	29,99		WNW	2	Cloudy.
22	7	0	47	59	30,23		W	1	Fine.
	2	0	61,5	60,5	30,20		SSW	2	Cloudy.
23	7	0	58	60	29,90		SSW	2	Cloudy.
	2	0	66	61,5	29,90		W	2	Cloudy.
24	7	0	55	61,5	30,25	0,063	NW	2	Cloudy.
	2	0	61	61,5	30,34		NW	2	Cloudy.
25	7	0	53	60	30,38		NW	1	Cloudy.
	2	0	60	61	30,35		NW	1	Cloudy.
26	7	0	48	59,5	30,42		WNW	1	Cloudy.
	2	0	60	60	30,42		N	1	Cloudy.
27	7	0	52	59	30,36		NE	1	Fair.
	2	0	56	59	30,30		ESE	1	Cloudy.
28	7	0	42	58	30,14				Foggy.
	2	0	56,5	59	30,05		W	1	Fine.
29	7	0	50	58	30,17		N	1	Cloudy.
	2	0	59,5	59	30,22		N	1	Fair.
30	7	0	52,5	58	30,25		NE	1	Cloudy.
	2	0	59,5	59,5	30,20		E	1	Fair.

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1790	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Oct. 1	7	0	46	58	30,03		E	1	Fine.
	2	0	57	59,5	29,95		E	1	Fine.
2	7	0	49	58	29,80		ENE	1	Cloudy.
	2	0	60	60	29,78		E	1	Fine.
3	7	0	56	59	29,77	0,083	E	1	Cloudy.
	2	0	64	60	29,82		S	1	Cloudy.
4	7	0	51,5	59	29,90		N	1	Cloudy.
	2	0	57	59 5	29,95		W	1	Cloudy.
5	7	0	52	59,5	30,00	0,401			Foggy.
	2	0	60	60,5	30,00		W	1	Fair.
6	7	0	54	60	29,80	0,132	SW	2	Cloudy.
	2	0	62	62	29,80		SW	2	Fair.
7	7	0	56	60,5	29,78	0,035	SSW	1	Cloudy.
	2	0	64,5	63	29,76		SW	1	Fine.
8	7	0	51	61,5	29,84		WNW	1	Fine.
	2	0	59	63,5	29,89		WNW	1	Fine.
9	7	0	52	61,5	29,89	0,101	N	1	Cloudy.
	2	0	54,5	63,5	29,94		NNW	1	Fine.
10	7	0	40	58	30,09		NE	1	Fine.
	2	0	50,5	59	30,08		N	1	Hazy.
11	7	0	39	56,5	30,11		N	1	Foggy.
	2	0	53,5	59	30,07		SSW	1	Fine.
12	7	0	51	57	29,86		WSW	1	Cloudy.
	2	0	58	57,5	29,66		SW	2	Cloudy.
13	7	0	53	56,5	29,71	0,073	SW	2	Fine.
	2	0	55,5	58	29,79		W	2	Fine.
14	7	0	52	56	29,80		SW	2	Cloudy.
	2	0	64,5	59,5	29,79		SW	2	Cloudy.
15	7	0	53,5	58,5	29,98		WSW	2	Fair.
	2	0	62	60,5	30,00		W	2	Cloudy.
16	7	0	45	58	30,34		W	1	Cloudy.
	2	0	53,5	60	30,40		E	1	Fine.

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1790	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Oct. 17	7	0	46	57,5	30,20		E	1	Fine.
	2	0	57	60,5	30,01		SSE.	1	Fine.
18	7	0	55	58	29,89		SW	2	Cloudy.
	2	0	55	60,5	29,86		W	1	Rain.
19	7	0	39	57,5	30,17	0,263	W	1	Fine.
	2	0	50	60	30,20		W	1	Fine.
20	7	0	41	57	30,05		E	1	Fine.
	2	0	55,5	59.5	29,96		E	1	Hazy.
21	7	0	52	58	29,93	0,020	E	1	Rain.
	2	0	60,5	62	29,87		E	1	Fine
22	7	0	54	60	29,75		E	1	Cloudy.
	2	0	62	62,5	29,70		E	1	Cloudy.
23	7	0	53	60,5	29,72		E	1	Cloudy.
	2	0	59,5	62,5	29,62		E	1	Cloudy.
24	7	0	51	60	29,72		SW	1	Cloudy.
	2	0	55,5	62,5	29,72		W	1	Cloudy.
25	7	0	50	60,5	29,70		WNW	1	Cloudy.
	2	0	56	62,5	29,77		WNW	1	Fair.
26	7	0	49	60	29,90		NE	1	Cloudy.
	2	0	56	62	29,90		NE	1	Cloudy.
27	7	0	46,5	60	29,85		NE	2	Cloudy.
	2	0	49	58,5	29,83		NE	2	Cloudy.
28	7	0	46	58	29,68		E	1	Cloudy.
	2	0	48	60	29,67		E	1	Cloudy.
29	7	0	46	58	29,77		E	2	Cloudy.
	2	0	48	60	29,83		E	2	Fair.
30	7	0	43	57	29,84		NE	1	Cloudy.
	2	0	45	57,5	29,83		NE	2	Cloudy.
31	7	0	40	55	29,86		NNE	1	Cloudy.
	2	0	45	58,5	29,90		N	1	Cloudy.

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1790	Time. H	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Nov. 1	7	0	43	54	29,83		SE	2	Cloudy.
	2	0	50	56,5	29,66		S	2	Rain.
2	7	0	45	54,5	29,34	0,619	W	1	Cloudy.
	2	0	52	58	29,48		W	2	Cloudy.
3	7	0	41	54,5	29,80		W	1	Fine.
	2	0	50	58,5	29,78		W	1	Cloudy.
4	7	0	44	55	29,69	0,170	W	1	Fine.
	2	0	51	59	29,70		W	1	Fine.
5	7	0	46	56	29,95		W	1	Fair.
	2	0	52,5	58	29,94		S	1	Cloudy.
6	7	0	51	57	29,74		E	2	Cloudy.
	2	0	53	58	29,68		SSE	2	Rain.
7	7	0	44	56	29,64	0,057	E	1	Cloudy.
	2	0	47	57	29,66		E	1	Cloudy.
8	7	0	40	55	29,85		E	1	Fine.
	2	0	48	58	29,87		E	2	Fine.
9	7	0	43	56	30,04		NE	1	Cloudy.
	2	0	47	58	30,09		ENE	1	Cloudy.
10	7	0	43	55,5	30,20		ENE	1	Cloudy.
	2	0	46	57	30,06		ENE	1	Cloudy.
11	7	0	47	56	29,96		NE	1	Cloudy.
	2	0	48	57	30,03		NE	1	Cloudy.
12	7	0	47	56	30,11		NE	1	Cloudy.
	2	0	47,5	57,5	30,11		NE	1	Cloudy.
13	7	0	42	55,5	30,16		NE	1	Cloudy.
	2	0	52,5	58	30,19		NE	1	Fine.
14	7	0	43	55	30,27		NE	1	Cloudy.
	2	0	47	57	30,26		NE	1	Cloudy.
15	7	0	40	54,5	30,30		NE	2	Cloudy.
	2	0	44	56	30 27		NE	2	Fine.
16	7	0	35,5	53	30,16		NE	2	Fine.
	2	0	43	55	30,11		ENE	2	Fine.

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1790	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Nov. 17	7	0	40,5	53	29,98		ENE	1	Cloudy.
	2	0	43,5	55,5	29,91		ENE	2	Fair.
18	7	0	34	52,5	29,71		W	1	Cloudy.
	2	0	38	53,5	29,55		SW	1	Fair.
19	7	0	46	53	29,13		S	2	Rain.
	2	0	44	55	29,10		SSW	2	Cloudy.
20	7	0	43	53	29,16	0,295	S	2	Fine.
	2	0	48	56	29,21		S	2	Rain.
21	7	0	44	53	29,02	0,150	SW	2	Fair.
	2	0	48	57	29,10		SW	1	Rain.
22	7	0	45	54	29,27	0,950	S	1	Cloudy.
	2	0	48,5	58,5	29,38		N	1	Cloudy.
23	7	0	46	55	29,61	0,115	N	1	Foggy.
	2	0	48	58	29,68		SW	1	Cloudy.
24	7	0	35	54	29,83		SW	1	Cloudy.
	2	0	44,5	56	29,85		W	1	Fine.
25	7	0	47	54,5	29,73		SW	2	Cloudy.
	2	0	50	57	29,58		S	2	Cloudy.
26	7	0	49	55	29,48	0,156	WSW	1	Cloudy.
	2	0	48	57,5	29,74		ENE	1	Cloudy.
27	7	0	36	54	30,21		NW	1	Fine.
	2	0	42	57	30,28		NW	1	Cloudy.
28	7	0	37	53,5	30,40		N	1	Fair.
	2	0	43	58	30,34		E	1	Cloudy.
29	7	0	34,5	53	30,14		E	1	Cloudy.
	2	0	35	54,5	30,05		E	1	Cloudy.
30	7	0	32	50	29,72		E	1	Cloudy.
	2	0	34	51,5	29,66		E	1	Cloudy.

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1790	Time. H.	M.	Therm. without °	Therm. within. °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Dec. 1	8	0	32	49	29,64		ESE	1	Cloudy.
	2	0	32	50,5	29,22		E	2	Snow.
2	8	0	43	51	29,31		WNW	2	Cloudy.
	2	0	46	54	29,59		WNW	2	Fair.
3	8	0	42,5	52	29,85		S	1	Rain.
	2	0	50	55	29,51		SSW	2	Cloudy.
4	8	0	42	53	29,94	0,220	W	1	Cloudy.
	2	0	44	56	29,97		W	1	Cloudy.
5	8	0	37	52	29,91		WNW	1	Cloudy.
	2	0	42	56	30,00		WNW	1	Cloudy.
6	8	0	36	52	30,36		NW	1	Cloudy.
	2	0	41	56,5	30,38		NW	1	Fine.
7	8	0	38	51,5	30,33		W	1	Cloudy.
	2	0	43,5	54,5	30,24		W	1	Cloudy.
8	8	0	47	54	30,15	0,105	NW	1	Cloudy.
	2	0	48	57	30,18		N	1	Cloudy.
9	8	0	47	54,5	30,18	0,023	NNW	1	Foggy.
	2	0	48,5	57,5	30,15		WNW	1	Cloudy.
10	8	0	47	56	30,19		W	1	Cloudy.
	2	0	50	58,5	30,19		W	1	Foggy.
11	8	0	49	56,5	29,96		W	2	Cloudy.
	2	0	52	58	29,85		SW	2	Rain.
12	8	0	47	54,5	30,06	0,015	W	1	Fine.
	2	0	43,5	58,5	30,08		W	1	Fine.
13	8	0	46,5	55,5	29,86	0,028	W	2	Cloudy.
	2	0	48	58	29,55		SSW	2	Cloudy.
14	8	0	39	55	29,85	0,444	W	2	Fine.
	2	0	43	57,5	30,00		WNW	2	Fine.
15	8	0	46	55	29,33	0,082	NW	2	Cloudy, much wind last night.
	2	0	48	57	29,40		W	2	Fair.
16	8	0	38	54	29,70		W	1	Cloudy.
	2	0	42	55,5	29,36		S	1	Rain.

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1790	Time. H.	M.	Therm. without °	Therm. within °	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Dec. 17	8	0	39	54,5	29,37	0,220	WSW	1	Fine.
	2	0	43	58	29,24		SW	1	Cloudy.
18	8	0	38	52	28,80	0,107	W	2	Cloudy, much wind last night.
	2	0	39	53,5	29,15		W	2	Cloudy.
19	8	0	31,5	51,5	29,55		SW	1	Fine.
	2	0	33	53,5	29,15		SE	2	Snow.
20	8	0	33	52,5	29,90	0,185	SW	1	Fine.
	2	0	34,5	53	30,06		WNW	1	Fine.
21	8	0	43	50	29,90		SW	2	Cloudy.
	2	0	50	53	29,79		SSW	2	Rain.
22	8	0	38	51,5	30,06	0,070	W	2	Fine.
	2	0	43	54,5	30,17		W	2	Fine.
23	8	0	38	52	29,74	0,136	W	2	Fair, much wind, thunder, and lightning last night.
	2	0	43,5	54	29,87		W	2	Fair.
24	8	0	37	52	30,24		W	1	Foggy.
	2	0	42	54,5	30,17		SW	1	Cloudy.
25	8	0	44	53,5	30,00	0,133	NW	1	Rain.
	2	0	44	54	30,08		N	1	Cloudy.
26	8	0	33	52	30,24		W	1	Foggy.
	2	0	35,5	53,5	30,22		SW	1	Cloudy.
27	8	0	30	49	30,04		WSW	1	Fair.
	2	0	38	53,5	29,90		SW	1	Cloudy.
28	8	0	33,5	50	29,97	0,020	WNW	1	Fine.
	2	0	36,5	54	30,07		NNW	1	Fine.
29	8	0	30	48,5	30,25		N	1	Fine.
	2	0	32	51,5	30,22		WSW	1	Fine.
30	8	0	37	48	29,94		S	2	Cloudy.
	2	0	39	50	29,84		SSW	2	Cloudy.
31	8	0	36	48	30,02	0,305	N	1	Cloudy.
	2	0	41	51,5	30,14		N	1	Cloudy.

	Therm without.			Therm. within.			Barometer.			Rain.
1790	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	°	°	°	°	°	°	Inches.	Inches.	Inches.	Inches.
January	52,5	32	40,9	56,5	50	53,1	30,47	29,27	30,07	0,967
February	55	33	43,8	59,5	52	54,9	30,62	29,88	30,25	0,115
March	56	33	45,4	59	52,5	56,8	30,65	29,83	30,26	0,122
April	58	35	44,0	59,5	51,5	55,5	30,30	29,38	29,86	1,470
May	66	47	56,3	66	58	60,9	30,14	29,50	29,90	2,898
June	86	53	60,5	73,5	60	63,1	30,35	29,49	30,03	0,708
July	73,5	53	62,2	67	61	63,5	30,20	29,29	29,84	1,700
August	77	51,5	63,4	73	61,5	65,2	30,16	29,64	29,97	1,991
September	68	45	56,6	65	58	60,8	30,42	29,31	30,00	0,368
October	64,5	39	52,4	63,5	55	59,5	30,40	29,62	29,89	1,108
November	53	32	44,3	59	50	55,4	30,40	29,02	29,81	2,512
December	52	30	40,9	58,5	48	53,6	30,38	28,80	29,88	2,093
Whole year			50,9			58,5			29,98	16,052

END OF PART I. OF VOL. LXXXI.